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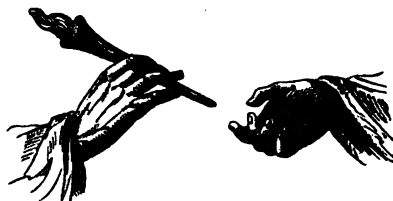
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THE  
PHILOSOPHY  
OF THE  
INDUCTIVE SCIENCES,  
FOUNDED UPON THEIR HISTORY.

BY WILLIAM WHEWELL, D.D.,  
MASTER OF TRINITY COLLEGE, CAMBRIDGE.

A NEW EDITION,  
WITH CORRECTIONS AND ADDITIONS, AND  
AN APPENDIX, CONTAINING  
PHILOSOPHICAL ESSAYS PREVIOUSLY PUBLISHED.

IN TWO VOLUMES.



*Λαμπάδια ἔχοντες διαδώσουσιν ἀλλήλοις.*



VOLUME THE SECOND.

---

LONDON:  
JOHN W. PARKER, WEST STRAND.

M.DCCC.XLVII.

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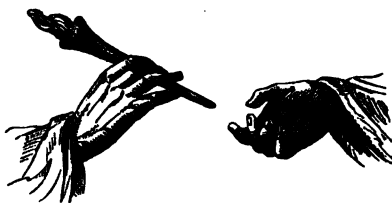
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**THE**  
**PHILOSOPHY**  
**OF THE**  
**INDUCTIVE SCIENCES.**

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**PART II.**  
**OF KNOWLEDGE.**

De Scientiis tum demum bene sperandum est, quando per SCALAM veram et per gradus continuos, et non intermissos aut hiulcos, a particularibus ascendetur ad Axiomatur minora, et deinde ad media, alia aliis superiora, et postremò demum ad generalissima.

In constituendo autem Axiomate, Forma INDUCTIONIS alia quam adhuc in usu fuit, excogitanda est; et quæ non ad Principia tantum (quæ vocant) probanda et invenienda, sed etiam ad Axiomata minora, et media, denique omnia.

BACON, *Nov. Org.*, Aph. civ. cv.

# BOOK XI.

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## OF THE CONSTRUCTION OF SCIENCE.

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### CHAPTER I.

#### OF TWO PRINCIPAL PROCESSES BY WHICH SCIENCE IS CONSTRUCTED.

To the subject of the present Book all that has preceded is subordinate and preparatory. The First Part of this work treated of Ideas: we now enter upon the Second Part, in which we have to consider the Knowledge which arises from them. It has already been stated that Knowledge requires us to possess both Facts and Ideas;—that every step in our knowledge consists in applying the ideas and conceptions furnished by our minds to the facts which observation and experiment offer to us. When our conceptions are clear and distinct, when our facts are certain and sufficiently numerous, and when the conceptions, being suited to the nature of the facts, are applied to them so as to produce an exact and universal accordance, we attain knowledge of a precise and comprehensive kind, which we may term *Science*. And we apply this term to our knowledge still more decidedly when, facts being thus included in exact and general propositions, such propositions are, in the same manner, included with equal rigour in propositions of a higher degree of generality; and these again in others of a still wider nature, so as to form a large and systematic whole.

But after thus stating, in a general way, the nature of science, and the elements of which it consists, we have been examining with a more close and extensive scrutiny, some of those elements; and we must now return to our main subject, and apply to it the results of our long investigation. We have been exploring the realm of Ideas; we have been passing in review the difficulties in which the workings of our own minds involve us when we would make our conceptions consistent with themselves: and we have endeavoured to get a sight of the true solutions of these difficulties. We have now to inquire how the results of these long and laborious efforts of thought find their due place in the formation of our knowledge. What do we gain by these attempts to make our notions distinct and consistent; and in what manner is the gain of which we thus become possessed, carried to the general treasure-house of our permanent and indestructible knowledge? After all this battling in the world of ideas, all this struggling with the shadowy and changing forms of intellectual perplexity, how do we secure to ourselves the fruits of our warfare, and assure ourselves that we have really pushed forwards the frontier of the empire of Science? It is by such an appropriation that the task which we have had in our hands during the last nine Books of this work, must acquire its real value and true place in our design.

In order to do this, we must reconsider, in a more definite and precise shape, the doctrine which has already been laid down;—that our knowledge consists in applying Ideas to Facts; and that the conditions of real knowledge are that the ideas be distinct and appropriate, and exactly applied to clear and certain facts. The steps by which our knowledge is advanced are those by which one or the other of these two processes is rendered more complete;—by which *conceptions* are *made*

*more clear* in themselves, or by which the conceptions more strictly *bind together the facts*. These two processes may be considered as together constituting the whole formation of our knowledge; and the principles which have been established in the preceding Books, bear principally upon the former of these two operations;—upon the business of elevating our conceptions to the highest possible point of precision and generality. But these two portions of the progress of knowledge are so clearly connected with each other, that we shall consider them in immediate succession. And having now to consider these operations in a more exact and formal manner than it was before possible to do, we shall designate them by certain constant and technical phrases. We shall speak of the two processes by which we arrive at science, as *the Explication of Conceptions* and *the Colligation of Facts*: we shall show how the discussions in which we have been engaged have been necessary in order to promote the former of these offices; and we shall endeavour to point out modes, maxims, and principles by which the second of the two tasks may also be furthered.

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## CHAPTER II.

### OF THE EXPLICATION OF CONCEPTIONS.

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#### SECT. I.—*Its historical Progress.*

1. WE have given the appellation of *Ideas* to certain comprehensive forms of thought,—as *space, number, cause, composition, resemblance*,—which we apply to the phenomena which we contemplate. But the special modifications of these ideas which are exemplified in

particular facts, we have termed *Conceptions*; as a *circle*, a *square number*, an *accelerating force*, a *neutral combination* of elements, a *genus*. Such Conceptions involve in themselves certain necessary and universal relations derived from the Ideas just enumerated; and these relations are an indispensable portion of the texture of our knowledge. But to determine the contents and limits of this portion of our knowledge, requires an examination of the Ideas and Conceptions from which it proceeds. The Conceptions must be, as it were, carefully *unfolded*, so as to bring into clear view the elements of truth with which they are marked from their ideal origin. This is one of the processes by which our knowledge is extended and made more exact; and this I shall describe as the *Explication of Conceptions*.

In the preceding Books we have discussed a great many of the Fundamental Ideas of the most important existing sciences. We have, in those Books, abundant exemplifications of the process now under our consideration. We shall here add a few general remarks, suggested by the survey which we have thus made.

2. Such discussions as those in which we have been engaged concerning our fundamental Ideas, have been the course by which, historically speaking, those Conceptions which the existing sciences involve have been rendered so clear as to be fit elements of exact knowledge. The disputes concerning the various kinds and measures of *Force* were an important part of the progress of the science of Mechanics. The struggles by which philosophers attained a right general conception of *plane*, of *circular*, of *elliptical Polarization*, were some of the most difficult steps in the modern discoveries of Optics. A Conception of the *Atomic Constitution* of bodies, such as shall include what we know, and assume nothing more, is even now a matter of conflict

among Chemists. The debates by which, in recent times, the Conceptions of *Species* and *Genera* have been rendered more exact, have improved the science of Botany: the imperfection of the science of Mineralogy arises in a great measure from the circumstance, that in that subject, the Conception of a *Species* is not yet fixed. In physiology, what a vast advance would that philosopher make, who should establish a precise, tenable, and consistent Conception of *Life*!

Thus discussions and speculations concerning the import of very abstract and general terms and notions, may be, and in reality have been, far from useless and barren. Such discussions arose from the desire of men to impress their opinions on others, but they had the effect of making the opinions much more clear and distinct. In trying to make others understand them, they learnt to understand themselves. Their speculations were begun in twilight, and ended in the full brilliance of day. It was not easily and at once, without expenditure of labour or time, that men arrived at those notions which now form the elements of our knowledge; on the contrary, we have, in the history of science, seen how hard discoverers, and the forerunners of discoverers, have had to struggle with the indistinctness and obscurity of the intellect, before they could advance to the critical point at which truth became clearly visible. And so long as, in this advance, some speculators were more forward than others, there was a natural and inevitable ground of difference of opinion, of argumentation, of wrangling. But the tendency of all such controversy is to diffuse truth and to dispel error. Truth is consistent, and can bear the tug of war; Error is incoherent, and falls to pieces in the struggle. True Conceptions can endure the sun, and become clearer as a fuller light is obtained; confused and inconsistent



notions vanish like visionary spectres at the break of a brighter day. And thus all the controversies concerning such Conceptions as science involves have ever ended in the establishment of the side on which the truth was found.

3. Indeed, so complete has been the victory of truth in most of these instances, that at present we can hardly imagine the struggle to have been necessary. The very essence of these triumphs is that they lead us to regard the views we reject as not only false, but inconceivable. And hence we are led rather to look back upon the vanquished with contempt than upon the victors with gratitude. We now despise those who in the Copernican controversy could not conceive the apparent motion of the sun on the heliocentric hypothesis;—or those who, in opposition to Galileo, thought that a uniform force might be that which generated a velocity proportional to the space;—or those who held there was something absurd in Newton's doctrine of the different refrangibility of differently coloured rays;—or those who imagined that when elements combine, their sensible qualities must be manifest in the compound;—or those who were reluctant to give up the distinction of vegetables into herbs, shrubs, and trees. We cannot help thinking that men must have been singularly dull of comprehension, to find a difficulty in admitting what is to us so plain and simple. We have a latent persuasion that we in their place should have been wiser and more clear-sighted;—that we should have taken the right side, and given our assent at once to the truth.

4. Yet in reality, such a persuasion is a mere delusion. The persons who, in such instances as the above, were on the losing side, were very far, in most cases, from being persons more prejudiced, or stupid, or narrow-minded, than the greater part of mankind now are;

and the cause for which they fought was far from being a manifestly bad one, till it had been so decided by the result of the war. It is the peculiar character of scientific contests, that what is only an epigram with regard to other warfare is a truth in this;—They who are defeated are really in the wrong. But they may, nevertheless, be men of great subtilty, sagacity, and genius; and we nourish a very foolish self-complacency when we suppose that we are their superiors. That this is so, is proved by recollecting that many of those who have made very great discoveries have laboured under the imperfection of thought which was the obstacle to the next step in knowledge. Though Kepler detected with great acuteness the Numerical Laws of the solar system, he laboured in vain to conceive the very simplest of the Laws of Motion by which the paths of the planets are governed. Though Priestley made some important steps in chemistry, he could not bring his mind to admit the doctrine of a general Principle of Oxidation. How many ingenious men in the last century rejected the Newtonian Attraction as an impossible chimera! How many more, equally intelligent, have, in the same manner, in our own time, rejected, I do not now mean as false, but as inconceivable, the doctrine of Luminiferous Undulations! To err in this way is the lot, not only of men in general, but of men of great endowments, and very sincere love of truth.

5. And those who liberate themselves from such perplexities, and who thus go on in advance of their age in such matters, owe their superiority in no small degree to such discussions and controversies as those to which we now refer. In such controversies, the conceptions in question are turned in all directions, examined on all sides; the strength and the weakness of the maxims which men apply to them are fully tested; the light of

the brightest minds is diffused to others. Inconsistency is unfolded into self-contradiction; axioms are built up into a system of necessary truths; and ready exemplifications are accumulated of that which is to be proved or disproved concerning the ideas which are the basis of the controversy.

The History of Mechanics from the time of Kepler to that of Lagrange, is perhaps the best exemplification of the mode in which the progress of a science depends upon such disputes and speculations as give clearness and generality to its elementary conceptions. This, it is to be recollected, is the kind of progress of which we are now speaking; and this is the principal feature in the portion of scientific history which we have mentioned. For almost all that was to be done by reference to observation, was executed by Galileo and his disciples. What remained was the task of generalization and simplification. And this was promoted in no small degree by the various controversies which took place within that period concerning mechanical conceptions:—as, for example, the question concerning the measure of the Force of Percussion;—the war of the *Vis Viva*;—the controversy of the Center of Oscillation;—of the independence of Statics and Dynamics;—of the principle of Least Action;—of the evidence of the Laws of Motion;—and of the number of Laws really distinct. None of these discussions was without its influence in giving generality and clearness to the mechanical ideas of mathematicians: and therefore, though remote from general apprehension, and dealing with very abstract notions, they were of eminent use in the perfecting the science of mechanics. Similar controversies concerning fundamental notions, those, for example, which Galileo himself had to maintain, were no less useful in the formation of the science of hydrostatics. And the like

struggles and conflicts, whether they take the form of controversies between several persons, or only operate in the efforts and fluctuations of the discoverer's mind, are always requisite before the conceptions acquire that clearness which makes them fit to appear in the enunciation of scientific truth.

This, then, is one object of the preceding Books;—to bring under the reader's notice the main elements of the controversies which have thus had so important a share in the formation of the existing body of science, and the decisions on the controverted points to which the mature examination of the subject has led; and thus to give an abundant exhibition of that step which we term the Explication of Conceptions.

#### SECT. II.—*Use of Definitions.*

6. The result of such controversies as we have been speaking of, often appears to be summed up in a *Definition*; and the controversy itself has often assumed the form of a battle of definitions. For example, the inquiry concerning the Laws of Falling Bodies led to the question whether the proper Definition of a *uniform force* is, that it generates a velocity proportional to the *space* from rest, or to the *time*. The controversy of the *Vis Viva* was, what was the proper Definition of the *measure of force*. A principal question in the classification of minerals is, what is the Definition of a *mineral species*. Physiologists have endeavoured to throw light on their subject, by Defining *organization*, or some similar term.

7. It is very important for us to observe, that these controversies have never been questions of insulated and *arbitrary* Definitions, as men seem often tempted to suppose them to have been. In all cases there is a tacit assumption of some Proposition which is to be expressed

by means of the Definition, and which gives it its importance. The dispute concerning the Definition thus acquires a real value, and becomes a question concerning true and false. Thus in the discussion of the question, What is a Uniform Force? it was taken for granted that "gravity is a uniform force:"—in the debate of the *Vis Viva*, it was assumed that "in the mutual action of bodies the whole effect of the force is unchanged:"—in the zoological definition of Species, (that it consists of individuals which have, or may have, sprung from the same parents,) it is presumed that "individuals so related resemble each other more than those which are excluded by such a definition;" or perhaps, that "species so defined have permanent and definite differences." A definition of Organization, or of any other term, which was not employed to express some principle, would be of no value.

The establishment, therefore, of a right Definition of a Term may be a useful step in the explication of our conceptions; but this will be the case then only when we have under our consideration some Proposition in which the Term is employed. For then the question really is, how the Conception shall be understood and defined in order that the Proposition may be true.

8. The establishment of a Proposition requires an attention to observed Facts, and can never be rightly derived from our Conceptions alone. We must hereafter consider the necessity which exists that the Facts should be rightly bound together, as well as that our Conceptions should be clearly employed, in order to lead us to real knowledge. But we may observe here that, in such cases at least as we are now considering, the two processes are co-ordinate. To unfold our Conceptions by the means of Definitions, has never been serviceable to science, except when it has been associated with an

immediate *use* of the Definitions. The endeavour to define a Uniform Force was combined with the assertion that "gravity is a uniform force:" the attempt to define Accelerating Force was immediately followed by the doctrine that "accelerating forces may be compounded:" the process of defining Momentum was connected with the principle that "momenta gained and lost are equal:" naturalists would have given in vain the Definition of Species which we have quoted, if they had not also given the "characters" of species so separated. Definition and Proposition are the two handles of the instrument by which we apprehend truth; the former is of no use without the latter. Definition may be the best mode of explaining our Conception, but that which alone makes it worth while to explain it in any mode, is the opportunity of using it in the expression of Truth. When a Definition is propounded to us as a useful step in knowledge, we are always entitled to ask what Principle it serves to enunciate. If there be no answer to this inquiry, we define and give clearness to our conceptions in vain. While we labour at such a task, we do but light up a vacant room;—we sharpen a knife with which we have nothing to cut;—we take exact aim, while we load our artillery with blank cartridge;—we apply strict rules of grammar to sentences which have no meaning.

If, on the other hand, we have under our consideration a proposition probably established, every step which we can make in giving distinctness and exactness to the Terms which this proposition involves, is an important step towards scientific truth. In such cases, any improvement in our Definition is a real advance in the explication of our Conception. The clearness of our Expressions casts a light upon the Ideas which we contemplate and convey to others.

9. But though *Definition* may be subservient to a

right explication of our conceptions, it is *not essential* to that process. It is absolutely necessary to every advance in our knowledge, that those by whom such advances are made should possess clearly the conceptions which they employ: but it is by no means necessary that they should unfold these conceptions in the words of a formal Definition. It is easily seen, by examining the course of Galileo's discoveries, that he had a distinct conception of the *Moving Force* which urges bodies downwards upon an inclined plane, while he still hesitated whether to call it *Momentum*, *Energy*, *Impetus*, or *Force*, and did not venture to offer a Definition of the thing which was the subject of his thoughts. The Conception of *Polarization* was clear in the minds of many optical speculators, from the time of Huyghens and Newton to that of Young and Fresnel. This Conception we have defined to be "Opposite properties depending upon opposite positions;" but this notion was, by the discoverers, though constantly assumed and expressed by means of superfluous hypotheses, never clothed in definite language. And in the mean time, it was the custom, among subordinate writers on the same subjects, to say, that the term *Polarization* had no definite meaning, and was merely an expression of our ignorance. The Definition which was offered by Haüy and others of a *Mineralogical Species*;—"The same elements combined in the same proportions, with the same fundamental form;"—was false, inasmuch as it was incapable of being rigorously applied to any one case; but this defect did not prevent the philosophers who propounded such a Definition from making many valuable additions to mineralogical knowledge, in the way of identifying some species and distinguishing others. The right Conception which they possessed in their minds prevented their being misled by their own very erroneous Defini-

tion. The want of any precise Definitions of *Strata*, and *Formations*, and *Epochs*, among geologists, has not prevented the discussions which they have carried on upon such subjects from being highly serviceable in the promotion of geological knowledge. For however much the apparent vagueness of these terms might leave their arguments open to cavil, there was a general understanding prevalent among the most intelligent cultivators of the science, as to what was meant in such expressions; and this common understanding sufficed to determine what evidence should be considered conclusive and what inconclusive, in these inquiries. And thus the distinctness of Conception, which is a real requisite of scientific progress, existed in the minds of the inquirers, although Definitions, which are a partial and accidental evidence of this distinctness, had not yet been hit upon. The idea had been developed in men's minds, although a clothing of words had not been contrived for it, nor, perhaps, the necessity of such a vehicle felt: and thus that essential condition of the progress of knowledge of which we are here speaking existed; while it was left to the succeeding speculators to put this unwritten Rule in the form of a verbal Statute.

10. Men are often prone to consider it as a thoughtless *omission* of an essential circumstance, and as a *neglect* which involves some blame, when knowledge thus assumes a form in which Definitions, or rather Conceptions, are implied but are not expressed. But in such a judgment, they assume *that* to be a matter of choice requiring attention only, which is in fact as difficult and precarious as any other portion of the task of discovery. To define, so that our Definition shall have any scientific value, requires no small portion of that sagacity by which truth is detected. As we have already said, Definitions and Propositions are co-ordinate in their use and in their



origin. In many cases, perhaps in most, the Proposition which contains a scientific truth, is apprehended with confidence, but with some vagueness and vacillation, before it is put in a positive, distinct, and definite form. It is thus known to be true, before it can be enunciated in terms each of which is rigorously defined. The business of Definition is part of the business of discovery. When it has been clearly seen what ought to be our Definition, it must be pretty well known what truth we have to state. The Definition, as well as the discovery, supposes a decided step in our knowledge to have been made. The writers on Logic in the middle ages, made *Definition* the last stage in the progress of knowledge; and in this arrangement at least, the history of science, and the philosophy derived from the history, confirm their speculative views. If the Explication of our Conceptions ever assume the form of a Definition, this will come to pass, not as an arbitrary process, or as a matter of course, but as the mark of one of those happy efforts of sagacity to which all the successive advances of our knowledge are owing.

### SECT. III.—*Use of Axioms.*

11. Our Conceptions, then, even when they become so clear as the progress of knowledge requires, are not adequately expressed, or necessarily expressed at all, by means of Definitions. We may ask, then, whether there is any *other mode* of expression in which we may look for the evidence and exposition of that peculiar exactness of thought which the formation of science demands. And in answer to this inquiry, we may refer to the previous discussions respecting many of the Fundamental Ideas of the sciences. It has there been seen that these Ideas involve many elementary truths which enter into the texture of our knowledge, introducing into it connex-

ions and relations of the most important kind, although these elementary truths cannot be deduced from any verbal definition of the idea. It has been seen that these elementary truths may often be enunciated by means of *Axioms*, stated in addition to, or in preference to, Definitions. For example, the Idea of Cause, which forms the basis of the science of Mechanics, makes its appearance in our elementary mechanical reasonings, not as a Definition, but by means of the Axioms that "Causes are measured by their effects," and that "Reaction is equal and opposite to action." Such Axioms, tacitly assumed or occasionally stated, as maxims of acknowledged validity, belong to all the Ideas which form the foundations of the sciences, and are constantly employed in the reasoning and speculations of those who think clearly on such subjects. It may often be a task of some difficulty to detect and enunciate in words the Principles which are thus, perhaps silently and unconsciously, taken for granted by those who have a share in the establishment of scientific truth: but inasmuch as these Principles are an essential element in our knowledge, it is very important to our present purpose to separate them from the associated materials, and to trace them to their origin. This accordingly I have attempted to do, with regard to a considerable number of the most prominent of such Ideas, in the preceding Books. The reader will there find many of these Ideas resolved into Axioms and Principles by means of which their effect upon the elementary reasonings of the various sciences may be expressed. That part of the Work is intended to form, in some measure, a representation of the Ideal Side of our physical knowledge;—a Table of those contents of our Conceptions which are not received directly from facts;—an exhibition of Rules to which we know that truth must conform.

SECT. IV.—*Clear and appropriate Ideas.*

12. In order, however, that we may see the necessary cogency of these rules, we must possess, clearly and steadily, the Ideas from which the rules flow. In order to perceive the necessary relations of the Circles of the Sphere, we must possess clearly the Idea of Solid Space :—in order that we may see the demonstration of the composition of forces, we must have the Idea of Cause moulded into a distinct Conception of Statical Force. This is that *Clearness of Ideas* which we stipulate for in any one's mind, as the first essential condition of his making any new step in the discovery of truth. And we now see what answer we are able to give, if we are asked for a Criterion of this Clearness of Idea. The Criterion is, that the person shall *see* the necessity of the Axioms belonging to each Idea;—shall accept them in such a manner as to perceive the cogency of the reasonings founded upon them. Thus a person has a clear Idea of Space who follows the reasonings of geometry and fully apprehends their conclusiveness. The Explication of Conceptions, which we are speaking of as an essential part of real knowledge, is the process by which we bring the Clearness of our Ideas to bear upon the Formation of our Knowledge. And this is done, as we have now seen, not always, nor generally, nor principally, by laying down a Definition of the Conception; but by acquiring such a possession of it in our minds as enables, indeed compels us, to admit, along with the Conception, all the Axioms and Principles which it necessarily implies, and by which it produces its effect upon our reasonings.

13. But in order that we may make any real advance in the discovery of truth, our Ideas must not only be clear, they must also be *appropriate*. Each science has for its basis a different class of Ideas; and the

steps which constitute the progress of one science can never be made by employing the Ideas of another kind of science. No genuine advance could ever be obtained in Mechanics by applying to the subject the Ideas of Space and Time merely:—no advance in Chemistry, by the use of mere Mechanical Conceptions:—no discovery in Physiology, by referring facts to mere Chemical and Mechanical Principles. **Mechanics** must involve the Conception of *Force*;—Chemistry, the Conception of *Elementary Composition*;—Physiology, the Conception of *Vital Powers*. Each science must advance by means of its *appropriate* Conceptions. Each has its own field, which extends as far as its principles can be applied. I have already noted the separation of several of these fields by the divisions of the preceding Books. The Mechanical, the Secondary Mechanical, the Chemical, the Classificatory, the Biological Sciences form so many great Provinces in the Kingdom of knowledge, each in a great measure possessing its own peculiar fundamental principles. Every attempt to build up a new science by the application of principles which belong to an old one, will lead to frivolous and barren speculations.

This truth has been exemplified in all the instances in which subtle speculative men have failed in their attempts to frame new sciences, and especially in the essays of the ancient schools of philosophy in Greece, as has already been stated in the History of Science. Aristotle and his followers endeavoured in vain to account for the mechanical relation of forces in the lever by applying the *inappropriate* geometrical conceptions of the properties of the circle:—they speculated to no purpose about the elementary composition of bodies, because they assumed the *inappropriate* conception of *likeness* between the elements and the compound, instead of the genuine notion of elements merely deter-

*mining* the qualities of the compound. And in like manner, in modern times, we have seen, in the history of the fundamental ideas of the physiological sciences, how all the *inappropriate* mechanical and chemical and other ideas which were applied in succession to the subject failed in bringing into view any genuine physiological truth.

14. That the real cause of the failure in the instances above mentioned lay in the *Conceptions*, is plain. It was not ignorance of the facts which in these cases prevented the discovery of the truth. Aristotle was as well acquainted with the fact of the proportion of the weights which balance on a lever as Archimedes was, although Archimedes alone gave the true mechanical reason for the proportion.

With regard to the doctrine of the four elements indeed, the inapplicability of the conception of composition of qualities, required, perhaps, to be proved by some reference to facts. But this conception was devised at first, and accepted by succeeding times, in a blind and gratuitous manner, which could hardly have happened if men had been awake to the necessary condition of our knowledge;—that the conceptions which we introduce into our doctrines are not arbitrary or accidental notions, but certain peculiar modes of apprehension strictly determined by the subject of our speculations.

15. It may, however, be said that this injunction that we are to employ *appropriate* Conceptions only in the formation of our knowledge, cannot be of practical use, because we can only determine what Ideas *are* appropriate, by finding that they truly combine the facts. And this is to a certain extent true. Scientific discovery must ever depend upon some happy thought, of which we cannot trace the origin;—some fortunate cast of intellect, rising above all rules. No maxims can be given which

inevitably lead to discovery. No precepts will elevate a man of ordinary endowments to the level of a man of genius: nor will an inquirer of truly inventive mind need to come to the teacher of inductive philosophy to learn how to exercise the faculties which nature has given him. Such persons as Kepler or Fresnel, or Brewster, will have their powers of discovering truth little augmented by any injunctions respecting Distinct and Appropriate Ideas; and such men may very naturally question the utility of rules altogether.

16. But yet the opinions which such persons may entertain, will not lead us to doubt concerning the value of the attempts to analyze and methodize the process of discovery. Who would attend to Kepler if he had maintained that the speculations of Francis Bacon were worthless? Notwithstanding what has been said, we may venture to assert that the maxim which points out the necessity of Ideas appropriate as well as clear, for the purpose of discovering truth, is not without its use. It may, at least, have a value as a caution or prohibition, and may thus turn us away from labours certain to be fruitless. We have already seen that this maxim, if duly attended to, would have at once condemned, as wrongly directed, the speculations of physiologists of the mathematical, mechanical, chemical, and vital-fluid schools; since the Ideas which the teachers of these schools introduce, cannot suffice for the purposes of physiology, which seeks truths respecting the vital powers. Again, it is clear from similar considerations that no definition of a mineralogical species by chemical characters alone can answer the end of science, since we seek to make mineralogy, not an analytical but a classificatory science\*. Even

\* This agrees with what M. Necker has well observed in his "*Règne Mineral*," that those who have treated mineralogy as a merely chemical science, have substituted the analysis of substances for the classification of individuals. See above, B. VIII. chap. iii.

before the appropriate conception is matured in men's minds so that they see clearly what it is, they may still have light enough to see what it is not.

17. Another result of this view of the necessity of appropriate Ideas, combined with a survey of the history of science is, that though for the most part, as we shall see, the progress of science consists in accumulating and combining Facts rather than in debating concerning Definitions; there are still certain periods when the *discussion* of Definitions may be the most useful mode of cultivating some special branch of science. This discussion is of course always to be conducted by the light of facts; and, as has already been said, along with the settlement of every good Definition will occur the corresponding establishment of some Proposition. But still at particular periods, the want of a Definition, or of the clear conceptions which Definition supposes, may be peculiarly felt. A good and tenable Definition of *Species* in Mineralogy would at present be perhaps the most important step which the science could make. A just conception of the nature of *Life*, (and if expressed by means of a Definition, so much the better,) can hardly fail to give its possessor an immense advantage in the speculations which now come under the consideration of physiologists. And controversies respecting Definitions, in these cases, and such as these, may be very far from idle and unprofitable.

Thus the knowledge that Clear and Appropriate Ideas are requisite for discovery, although it does not lead to any very precise precepts, or supersede the value of natural sagacity and inventiveness, may still be of use to us in our pursuit after truth. It may show us what course of research is, in each stage of science, recommended by the general analogy of the history of knowledge; and it may both save us from hopeless and barren paths of speculation, and make us advance with more courage and

confidence, to know that we are looking for discoveries in the manner in which they have always hitherto been made.

SECT. V.—*Accidental Discoveries.*

18. Another consequence follows from the views presented in this Chapter, and it is the last I shall at present mention. *No scientific discovery* can, with any justice, be considered *due to accident*. In whatever manner facts may be presented to the notice of a discoverer, they can never become the materials of exact knowledge, except they find his mind already provided with precise and suitable conceptions by which they may be analyzed and connected. Indeed, as we have already seen, facts cannot be observed as Facts, except in virtue of the Conceptions which the observer\* himself unconsciously supplies; and they are not Facts of Observation for any purpose of Discovery, except these familiar and unconscious acts of thought be themselves of a just and precise kind. But supposing the Facts to be adequately observed, they can never be combined into any new Truth, except by means of some new Conceptions, clear and appropriate, such as I have endeavoured to characterize. When the observer's mind is prepared with such instruments, a very few facts, or it may be a single one, may bring the process of discovery into action. But in such cases, this previous condition of the intellect, and not the single fact, is really the main and peculiar cause of the success. The fact is merely the occasion by which the engine of discovery is brought into play sooner or later. It is, as I have elsewhere said, only the spark which discharges a gun already loaded and pointed; and there is little propriety in speaking of such an accident as the cause why the bullet hits the mark. If it were true that

\* Book I. c. ii.



the fall of an apple was the occasion of Newton's pursuing the train of thought which led to the doctrine of universal gravitation, the habits and constitution of Newton's intellect, and not the apple, were the real source of this great event in the progress of knowledge. The common love of the marvellous, and the vulgar desire to bring down the greatest achievements of genius to our own level, may lead men to ascribe such results to any casual circumstances which accompany them; but no one who fairly considers the real nature of great discoveries, and the intellectual processes which they involve, can seriously hold the opinion of their being the effect of accident.

19. Such accidents never happen to common men. Thousands of men, even of the most inquiring and speculative men, had seen bodies fall; but who, except Newton, ever followed the accident to such consequences? And in fact, how little of his train of thought was contained in, or even directly suggested by, the fall of the apple! If the apple fall, said the discoverer, why should not the moon, the planets, the satellites, fall? But how much previous thought,—what a steady conception of the universality of the laws of motion gathered from other sources,—were requisite, that the inquirer should see any connexion in these cases! Was it by accident that he saw in the apple an image of the moon, and of every body in the solar system?

20. The same observations may be made with regard to the other cases which are sometimes adduced as examples of accidental discovery. It has been said, "By the accidental placing of a rhomb of calcareous spar upon a book or line Bartholinus discovered the property of the *Double Refraction* of light." But Bartholinus could have seen no such consequence in the accident if he had not previously had a clear conception of *single*

*refraction*. A lady, in describing an optical experiment which had been shown her, said of her teacher, "He told me to *increase and diminish the angle of refraction*, and at last I found that he only meant me to move my head up and down." At any rate, till the lady had acquired the notions which the technical terms convey, she could not have made Bartholinus's discovery by means of his accident. "By accidentally combining two rhombs in different positions," it is added, "Huyghens discovered the *Polarization of Light*." Supposing that this experiment had been made without design, what Huyghens really observed, was that the images appeared and disappeared alternately as he turned one of the rhombs round. But was it an easy or an obvious business to analyze this curious alternation into the circumstances of the rays of light having *sides*, as Newton expressed it, and into the additional hypotheses which are implied in the term "polarization?" Those will be able to answer this question, who have found how far from easy it is to understand clearly what is meant by "polarization" in this case, now that the property is fully established. Huyghens's success depended on his clearness of thought, for this enabled him to perform the intellectual analysis, which never would have occurred to most men, however often they had "accidentally combined two rhombs in different positions. "By accidentally looking through a prism of the same substance, and turning it round, Malus discovered the polarization of light by reflection." Malus saw that, in some positions of the prism, the light reflected from the windows of the Louvre thus seen through the prism, became dim. A common man would have supposed this dimness the result of accident; but Malus's mind was differently constituted and disciplined. He considered the position of the window, and of the prism; repeated the experiment

over and over; and in virtue of the eminently distinct conceptions of space which he possessed, resolved the phenomena into its geometrical conditions. A believer in accident would not have sought them; a person of less clear ideas would not have found them. A person must have a strange confidence in the virtue of chance, and the worthlessness of intellect, who can say that "in all these fundamental discoveries appropriate ideas had no share," and that the discoveries "might have been made by the most ordinary observers."

21. I have now, I trust, shown in various ways, how the *Explication of Conceptions*, including in this term their clear developement from Fundamental Ideas in the discoverer's mind, as well as their precise expression in the form of Definitions or Axioms, when that can be done, is an essential part in the establishment of all exact and general physical truths. In doing this, I have endeavoured to explain in what sense the possession of clear and appropriate ideas is a main requisite for every step in scientific discovery. That it is far from being the only step, I shall soon have to show; and if any obscurity remain on the subject treated of in the present chapter, it will, I hope, be removed when we have examined the other elements which enter into the constitution of our knowledge.

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### CHAPTER III.

#### OF FACTS AS THE MATERIALS OF SCIENCE.

1. WE have now to examine how Science is built up by the combination of Facts. In doing this, we suppose that we have already obtained a supply of definite and certain Facts, free from obscurity and doubt. We

must, therefore, first consider under what conditions Facts can assume this character.

When we inquire what Facts are to be made the materials of Science, perhaps the answer which we should most commonly receive would be, that they must be *True Facts*, as distinguished from any mere inferences or opinions of our own. We should probably be told that we must be careful in such a case to consider as Facts, only what we really observe;—that we must assert only what we see; and believe nothing except upon the testimony of our senses.

But such maxims are far from being easy to apply, as a little examination will convince us.

2. It has been explained, in the preceding part of this work, that all perception of external objects and occurrences involves an active as well as a passive process of the mind;—includes not only Sensations, but also Ideas by which Sensations are bound together, and have a unity given to them. From this it follows, that there is a difficulty in separating in our perceptions what we receive from without, and what we ourselves contribute from within;—what we perceive, and what we infer. In many cases, this difficulty is obvious to all: as, for example, when we witness the performances of a juggler or a ventriloquist. In these instances, we imagine ourselves to see and to hear what certainly we do not see and hear. The performer takes advantage of the habits by which our minds supply interruptions and infer connexions; and by giving us fallacious indications, he leads us to perceive as an actual fact, what does not happen at all. In these cases, it is evident that we ourselves assist in making the fact; for we make one which does not really exist. In other cases, though the fact which we perceive be true, we can easily see that a large portion of the perception is our own act; as when, from

the sight of a bird of prey we infer a carcase, or when we read a half-obliterated inscription. In the latter case, the mind supplies the meaning, and perhaps half the letters; yet we do not hesitate to say that we actually *read* the inscription. Thus, in many cases, our own inferences and interpretations enter into our facts. But this happens in many instances in which it is at first sight less obvious. When any one has seen an oak-tree blown down by a strong gust of wind, he does not think of the occurrence any otherwise than as a *Fact* of which he is assured by his senses. Yet by what sense does he perceive the Force which he thus supposes the wind to exert? By what sense does he distinguish an Oak-tree from all other trees? It is clear upon reflection, that in such a case, his own mind supplies the conception of extraneous impulse and pressure, by which he thus interprets the motions observed, and the distinction of different kinds of trees, according to which he thus names the one under his notice. The Idea of Force, and the idea of definite Resemblances and Differences, are thus combined with the impressions on our senses, and form an undistinguished portion of that which we consider as the Fact. And it is evident that we can in no other way perceive Force, than by seeing motion; and cannot give a Name to any object, without not only seeing a difference of single objects, but supposing a difference of classes of objects. When we speak as if we saw impulse and attraction, things and classes, we really see only objects of various forms and colours, more or less numerous, variously combined. But do we really perceive so much as this? When we see the form, the size, the number, the motion of objects, are these really mere impressions on our senses, unmodified by any contribution or operation of the mind itself? A very little attention will suffice to convince us that this

is not the case. When we see a windmill turning, it may happen, as we have elsewhere noticed\*, that we mistake the direction in which the sails turn: when we look at certain diagrams, they may appear either convex or concave: when we see the moon first in the horizon and afterwards high up in the sky, we judge her to be much larger in the former than in the latter position, although to the eye she subtends the same angle. And in these cases and the like, it has been seen that the error and confusion which we thus incur arise from the mixture of acts of the mind itself with impressions on the senses. But such acts are, as we have also seen, *inseparable* portions of the process of perception. A certain activity of the mind is involved, not only in seeing objects erroneously, but in seeing them at all. With regard to solid objects, this is generally acknowledged. When we seem to see an edifice occupying space in all dimensions, we really see only a representation of it as it appears referred by perspective to a surface. The inference of the solid form is an operation of our own, alike when we look at a reality and when we look at a picture. But we may go further. Is plane Figure really a mere Sensation? If we look at a decagon, do we see at once that it has ten sides, or is it not necessary for us to count them: and is not counting an act of the mind? All objects are seen in space; all objects are seen as one or many: but are not the Idea of Space and the Idea of Number requisite in order that we may thus apprehend what we see? That these Ideas of Space and Number involve a connexion derived from the mind, and not from the senses, appears, as we have already seen, from this, that those Ideas afford us the materials of universally and necessary truths:—such truths as the senses cannot possibly supply. And thus,

\* Book II. c. vi. sect. 6.

even the perception of such facts as the size, shape, and number of objects, cannot be said to be impressions of sense, distinct from all acts of mind, and cannot be expected to be free from error on the ground of their being mere observed Facts.

Thus the difficulty which we have been illustrating, of distinguishing Facts from inferences and from interpretations of facts, is not only great, but amounts to an impossibility. The separation at which we aimed in the outset of this discussion, and which was supposed to be necessary in order to obtain a firm groundwork for science, is found to be unattainable. We cannot obtain a sure basis of Facts, by rejecting all inferences and judgments of our own, for such inferences and judgments form an unavoidable element in all Facts. We cannot exclude our Ideas from our Perceptions, for our Perceptions involve our Ideas.

3. But still, it cannot be doubted that in selecting the Facts which are to form the foundation of Science, we must reduce them to their most simple and certain form; and must reject everything from which doubt or error may arise. Now since this, it appears, cannot be done, by rejecting the Ideas which all Facts involve, in what manner are we to conform to the obvious maxim, that the Facts which form the basis of Science must be perfectly definite and certain?

The analysis of facts into Ideas and Sensations, which we have so often referred to, suggests the answer to this inquiry. We are not able, nor need we endeavour, to exclude Ideas from our Facts; but we may be able to discern, with perfect distinctness, the Ideas which we include. We cannot observe any phenomena without applying to them such Ideas as Space and Number, Cause and Resemblance, and usually, several others; but we may avoid applying these Ideas in a wavering or obscure

manner, and confounding Ideas with one another. We cannot read any of the inscriptions which nature presents to us, without interpreting them by means of some language which we ourselves are accustomed to speak; but we may make it our business to acquaint ourselves perfectly with the language which we thus employ, and to interpret it according to the rigorous rules of grammar and analogy.

This maxim, that when Facts are employed as the basis of Science, we must distinguish clearly the Ideas which they involve, and must apply these in a distinct and rigorous manner, will be found to be a more precise guide than we might perhaps at first expect. We may notice one or two Rules which flow from it.

4. In the first place, Facts, when used as the materials of physical Science, must be *referred to Conceptions of the Intellect only*, all emotions of fear, admiration, and the like, being rejected or subdued. Thus, the observations of phenomena which are related as portents and prodigies, striking terror and boding evil, are of no value for purposes of science. The tales of armies seen warring in the sky, the sound of arms heard from the clouds, fiery dragons, chariots, swords seen in the air, may refer to meteorological phenomena; but the records of phenomena observed in the state of mind which these descriptions imply can be of no scientific value. We cannot make the poets our observers.

Armorum sonitum toto Germania cœlo  
Audiit; insolitis tremuerunt motibus Alpes.  
Vox quoque per lucos vulgo exaudita silentis  
Ingens, et simulacra modis pallentia miris  
Visa sub obscurum noctis: pecudesque locutæ.

The mixture of fancy and emotion with the observation of facts has often disfigured them to an extent which is too familiar to all to need illustration. We have an



example of this result, in the manner in which Comets are described in the treatises of the middle ages. In such works, these bodies are regularly distributed into several classes, accordingly as they assume the form of a sword, of a spear, of a cross, and so on. When such resemblances had become matters of interest, the impressions of the senses were governed, not by the rigorous conceptions of form and colour, but by these assumed images; and under these circumstances, we can attach little value to the statement of what was seen.

In all such phenomena, the reference of the objects to the exact Ideas of Space, Number, Position, Motion, and the like, is the first step of Science: and accordingly, this reference was established at an early period in those sciences which made an early progress, as, for instance, astronomy. Yet even in astronomy there appears to have been a period when the predominant conceptions of men in regarding the heavens and the stars pointed to mythical story and supernatural influence, rather than to mere relations of space, time, and motion: and of this primeval condition of those who gazed at the stars, we seem to have remnants in the Constellations, in the mythological Names of the Planets, and in the early prevalence of Astrology. It was only at a later period, when men had begun to measure the places, or at least to count the revolutions of the stars, that astronomy had its birth.

5. And thus we are led to another Rule:—that in collecting Facts which are to be made the basis of Science, the Facts are to be observed, as far as possible, *with reference to place, figure, number, motion*, and the like Conceptions; which, depending upon the Ideas of Space and Time, are the most universal, exact, and simple of our conceptions. It was by early attention to these relations in the case of the heavenly bodies, that

the ancients formed the science of Astronomy: it was by not making precise observations of this kind in the case of terrestrial bodies, that they failed in framing a science of the Mechanics of Motion. They succeeded in Optics as far as they made observations of this nature; but when they ceased to trace the geometrical paths of rays in the actual experiment, they ceased to go forwards in the knowledge of this subject.

6. But we may state a further Rule:—that though these relations of Time and Space are highly important in almost all Facts, we are not to confine ourselves to these: but are to consider the phenomena *with reference to other Conceptions also*: it being always understood that these conceptions are to be made as exact and rigorous as those of geometry and number. Thus the science of Harmonics arose from considering sounds with reference to *Concords* and *Discords*; the science of Mechanics arose from not only observing motions as they take place in Time and Space, but further, referring them to *Force* as their *Cause*. And in like manner, other sciences depend upon other Ideas, which, as I have endeavoured to show, are not less fundamental than those of Time and Space; and like them, capable of leading to rigorous consequences.

7. Thus the Facts which we assume as the basis of Science are to be freed from all the mists which imagination and passion throw round them; and to be separated into those elementary Facts which exhibit simple and evident relations of Time, or Space, or Cause, or some other Ideas equally clear. We resolve the complex appearances which nature offers to us, and the mixed and manifold modes of looking at these appearances which rise in our thoughts, into limited, definite, and clearly-understood portions. This process we may term the *Decomposition of Facts*. It is the beginning

of exact knowledge,—the first step in the formation of all Science. This Decomposition of Facts into Elementary Facts, clearly understood and surely ascertained, must precede all discovery of the laws of nature.

8. But though this step is necessary, it is not infallibly sufficient. It by no means follows that when we have thus decomposed Facts into Elementary Truths of observation, we shall soon be able to combine these, so as to obtain Truths of a higher and more speculative kind. We have examples which show us how far this is from being a necessary consequence of the former step. Observations of the weather, made and recorded for many years, have not led to any general truths, forming a science of Meteorology: and although great numerical precision has been given to such observations by means of barometers, thermometers, and other instruments, still, no general laws regulating the cycles of change of such phenomena have yet been discovered. In like manner the faces of crystals, and the sides of the polygons which these crystals form, were counted, and thus numerical facts were obtained, perfectly true and definite, but still of no value for purposes of science. And when it was discovered what Element of the form of crystals it was important to observe and measure, namely, the Angle made by two faces with each other, this discovery was a step of a higher order, and did not belong to that department, of mere exact observation of manifest Facts, with which we are here concerned.

9. When the Complex Facts which nature offers to us are thus decomposed into Simple Facts, the decomposition, in general, leads to the introduction of *Terms* and *Phrases*, more or less technical, by which these Simple Facts are described. When Astronomy was thus made a science of measurement, the things measured were soon described as *Hours*, and *Days*, and

*Cycles, Altitude and Declination, Phases and Aspects.* In the same manner, in Music, the concords had names assigned them, as *Diapente, Diatessaron, Diapason*; in studying Optics, the *Rays* of light were spoken of as having their course altered by *Reflexion* and *Refraction*; and when useful observations began to be made in Mechanics, the observers spoke of *Force, Pressure, Momentum, Inertia*, and the like.

10. When we take phenomena in which the leading Idea is Resemblance, and resolve them into precise component Facts, we obtain some kind of Classification; as, for instance, when we lay down certain Rules by which particular trees, or particular animals are to be known. This is the earliest form of Natural History; and the Classification which it involves is that which corresponds, nearly or exactly, with the usual Names of the objects thus classified.

11. Thus the first attempts to render observation certain and exact, lead to a decomposition of the obvious facts into Elementary Facts, connected by the Ideas of Space, Time, Number, Cause, Likeness, and others: and into a Classification of the Simple Facts, more or less just, and marked by Names either common or technical. Elementary Facts, and Individual Objects, thus observed and classified, form the materials of Science; and any improvement in Classification or Nomenclature, or any discovery of a Connexion among the materials thus accumulated, leads us fairly within the precincts of Science. We must now, therefore, consider the manner in which Science is built up of such materials;—the process by which they are brought into their places, and the texture of the bond which unites and cements them.

## CHAPTER IV.

## OF THE COLLIGATION OF FACTS.

1. FACTS such as the last Chapter speaks of are, by means of such Conceptions as are described in the preceding Chapter, bound together so as to give rise to those general Propositions of which Science consists. Thus the Facts that the planets revolve about the sun in certain periodic times and at certain distances, are included and connected in Kepler's Law, by means of such Conceptions as the *squares of numbers*, the *cubes of distances*, and the *proportionality* of these quantities. Again the existence of this proportion in the motions of any two planets, forms a set of Facts which may all be combined by means of the Conception of a certain *central accelerating force*, as was proved by Newton. The whole of our physical knowledge consists in the establishment of such propositions; and in all such cases, Facts are bound together by the aid of suitable Conceptions. This part of the formation of our knowledge I have called the *Colligation of Facts*: and we may apply this term to every case in which, by an act of the intellect, we establish a precise connexion among the phenomena which are presented to our senses. The knowledge of such connexions, accumulated and systematized, is Science. On the steps by which science is thus collected from phenomena we shall proceed now to make a few remarks.

2. Science begins with *Common Observation* of facts, in which we are not conscious of any peculiar discipline or habit of thought exercised in observing. Thus the common perceptions of the appearances and recurrences of the celestial luminaries, were the first steps of Astro-

nomy: the obvious cases in which bodies fall or are supported, were the beginning of Mechanics; the familiar aspects of visible things, were the origin of Optics; the usual distinctions of well-known plants, first gave rise to Botany. Facts belonging to such parts of our knowledge are noticed by us, and accumulated in our memories, in the common course of our habits, almost without our being aware that we are observing and collecting facts. Yet such facts may lead to many scientific truths; for instance, in the first stages of Astronomy (as we have shown in the *History*) such facts lead to Methods of Intercalation and Rules of the Recurrence of Eclipses. In succeeding stages of science, more especial attention and preparation on the part of the observer, and a selection of certain *kinds* of facts, becomes necessary; but there is an early period in the progress of knowledge at which man is a physical philosopher, without seeking to be so, or being aware that he is so.

3. But in all stages of the progress, even in that early one of which we have just spoken, it is necessary, in order that the facts may be fit materials of any knowledge, that they should be decomposed into Elementary Facts, and that these should be observed with precision. Thus, in the first infancy of astronomy, the recurrence of phases of the moon, of places of the sun's rising and setting, of planets, of eclipses, was observed to take place at intervals of certain definite numbers of days, and in a certain exact order; and thus it was, that the observations became portions of astronomical science. In other cases, although the facts were equally numerous, and their general aspect equally familiar, they led to no science, because their exact circumstances were not apprehended. A vague and loose mode of looking at facts very easily observable, left men for a long time under the belief that a body, ten times as heavy as another,

falls ten times as fast;—that objects immersed in water are always magnified, without regard to the form of the surface;—that the magnet exerts an irresistible force;—that crystal is always found associated with ice;—and the like. These and many others are examples how blind and careless man can be, even in observation of the plainest and commonest appearances; and they show us that the mere faculties of perception, although constantly exercised upon innumerable objects, may long fail in leading to any exact knowledge.

4. If we further inquire what was the favourable condition through which some special classes of facts were, from the first, fitted to become portions of science, we shall find it to have been principally this;—that these facts were considered with reference to the Ideas of Time, Number, and Space, which are Ideas possessing peculiar definiteness and precision; so that with regard to them, confusion and indistinctness are hardly possible. The interval from new moon to new moon was always a particular number of days: the sun in his yearly course rose and set near to a known succession of distant objects: the moon's path passed among the stars in a certain order:—these are observations in which mistake and obscurity are not likely to occur, if the smallest degree of attention is bestowed upon the task. To count a number is, from the first opening of man's mental faculties, an operation which no science can render more precise. The relations of space are nearest to those of number in obvious and universal evidence. Sciences depending upon these Ideas arise with the first dawn of intellectual civilization. But few of the other Ideas which man employs in the acquisition of knowledge possess this clearness in their common use. The Idea of *Resemblance* may be noticed, as coming next to those of Space and Number in original precision; and the

Idea of *Cause*, in a certain vague and general mode of application, sufficient for the purposes of common life, but not for the ends of science, exercises a very extensive influence over men's thoughts. But the other Ideas on which science depends, with the Conceptions which arise out of them, are not unfolded till a much later period of intellectual progress; and therefore, except in such limited cases as I have noticed, the observations of common spectators and uncultivated nations, however numerous or varied, are of little or no effect in giving rise to Science.

5. Let us now suppose that, besides common everyday perception of facts, we turn our attention to some other occurrences and appearances, with a design of obtaining from them speculative knowledge. This process is more peculiarly called *Observation*, or, when we ourselves occasion the facts, *Experiment*. But the same remark which we have already made, still holds good here. These facts can be of no value, except they are resolved into those exact Conceptions which contain the essential circumstances of the case. They must be determined, not indeed necessarily, as has sometimes been said, "according to Number, Weight, and Measure;" for, as we have endeavoured to show in the preceding Books\*, there are many other Conceptions to which phenomena may be subordinated, quite different from these, and yet not at all less definite and precise. But in order that the facts obtained by observation and experiment may be capable of being used in furtherance of our exact and solid knowledge, they must be apprehended and analyzed according to some Conceptions which, applied for this purpose, give distinct and definite results, such as can be steadily taken hold of and reasoned from; that is, the facts must be referred to Clear and Appro-



priate Ideas, according to the manner in which we have already explained this condition of the derivation of our knowledge. The phenomena of light, when they are such as to indicate sides in the ray, must be referred to the Conception of *polarization*; the phenomena of mixture, when there is an alteration of qualities as well as quantities, must be combined by a Conception of *elementary composition*. And thus, when mere position, and number, and resemblance, will no longer answer the purpose of enabling us to connect the facts, we call in other Ideas, in such cases more efficacious, though less obvious.

6. But how are we, in these cases, to discover such Ideas, and to judge which will be efficacious, in leading to a scientific combination of our experimental data? To this question, we must in the first place answer, that the first and great instrument by which facts, so observed with a view to the formation of exact knowledge, are combined into important and permanent truths, is that peculiar Sagacity which belongs to the genius of a Discoverer; and which, while it supplies those distinct and appropriate Conceptions which lead to its success, cannot be limited by rules, or expressed in definitions. It would be difficult or impossible to describe in words the habits of thought which led Archimedes to refer the conditions of equilibrium on the lever to the Conception of *pressure*, while Aristotle could not see in them anything more than the results of the strangeness of the properties of the circle;—or which impelled Pascal to explain by means of the Conception of the *weight of air*, the facts which his predecessors had connected by the notion of nature's horror of a vacuum;—or which caused Vitello and Roger Bacon to refer the magnifying power of a convex lens to the bending of the rays of light towards the perpendicular by *refraction*, while

others conceived the effect to result from the matter of medium, with no consideration of its form. These are what are commonly spoken of as felicitous and inexplicable strokes of inventive talent; and such, no doubt, they are. No rules can ensure to us similar success in new cases; or can enable men who do not possess similar endowments, to make like advances in knowledge.

7. Yet still, we may do something in tracing the process by which such discoveries are made; and this it is here our business to do. We may observe that these, and the like discoveries, are not improperly described as happy *Guesses*; and that *Guesses*, in these as in other instances, imply various suppositions made, of which some one turns out to be the right one. We may, in such cases, conceive the discoverer as inventing and trying many conjectures, till he finds one which answers the purpose of combining the scattered facts into a single rule. The discovery of general truths from special facts is performed, commonly at least, and more commonly than at first appears, by the use of a series of Suppositions, or *Hypotheses*, which are looked at in quick succession, and of which the one which really leads to truth is rapidly detected, and when caught sight of, firmly held, verified, and followed to its consequences. In the minds of most discoverers, this process of invention, trial, and acceptance or rejection of the hypothesis, goes on so rapidly that we cannot trace it in its successive steps. But in some instances, we can do so; and we can also see that the other examples of discovery do not differ essentially from these. The same intellectual operations take place in other cases, although this often happens so instantaneously that we lose the trace of the progression. In the discoveries made by Kepler, we have a curious and memorable exhibition of this process in its details. Thanks to his communicative disposi-

tion, we know that he made nineteen hypotheses with regard to the motion of Mars, and calculated the results of each, before he established the true doctrine, that the planet's path is an ellipse. We know, in like manner, that Galileo made wrong suppositions respecting the laws of falling bodies, and Mariotte, concerning the motion of water in a siphon, before they hit upon the correct view of these cases.

8. But it has very often happened in the history of science, that the erroneous hypotheses which preceded the discovery of the truth have been made, not by the discoverer himself, but by his precursors; to whom he thus owed the service, often an important one in such cases, of exhausting the most tempting forms of error. Thus the various fruitless suppositions by which Kepler endeavoured to discover the law of refraction, led the way to its real detection by Snell; Kepler's numerous imaginations concerning the forces by which the celestial motions are produced,—his "physical reasonings" as he termed them,—were a natural prelude to the truer physical reasonings of Newton. The various hypotheses by which the suspension of vapour in air had been explained, and their failure, left the field open for Dalton with his doctrine of the mechanical mixture of gases. In most cases, if we could truly analyze the operation of the thoughts of those who make, or who endeavour to make discoveries in science, we should find that many more suppositions pass through their minds than those which are expressed in words; many a possible combination of conceptions is formed and soon rejected. There is a constant invention and activity, a perpetual creating and selecting power at work, of which the last results only are exhibited to us. Trains of hypotheses are called up and pass rapidly in review; and the judgment makes its choice from the varied group.

9. It would, however, be a great mistake to suppose that the hypotheses, among which our choice thus lies, are constructed by an enumeration of obvious cases, or by a wanton alteration of relations which occur in some first hypothesis. It may, indeed, sometimes happen that the proposition which is finally established is such as may be formed, by some slight alteration, from those which are justly rejected. Thus Kepler's elliptical theory of Mars's motions, involved relations of lines and angles much of the same nature as his previous false suppositions: and the true law of refraction so much resembles those erroneous ones which Kepler tried, that we cannot help wondering how he chanced to miss it. But it more frequently happens that new truths are brought into view by the application of new Ideas, not by new modifications of old ones. The cause of the properties of the Lever was learnt, not by introducing any new *geometrical* combination of lines and circles, but by referring the properties to genuine *mechanical* Conceptions. When the Motions of the Planets were to be explained, this was done, not by merely improving the previous notions, of cycles of time, but by introducing the new conception of *epicycles* in space. The doctrine of the Four Simple Elements was expelled, not by forming any new scheme of elements which should impart, according to new rules, their sensible qualities to their compounds, but by considering the elements of bodies as *neutralizing* each other. The Fringes of Shadows could not be explained by ascribing new properties to the single rays of light, but were reduced to law by referring them to the *interference* of several rays.

Since the true supposition is thus very frequently something altogether diverse from all the obvious conjectures and combinations, we see here how far we are from being able to reduce discovery to rule, or to give

any precepts by which the want of real invention and sagacity shall be supplied. We may warn and encourage these faculties when they exist, but we cannot create them, or make great discoveries when they are absent.

10. The Conceptions which a true theory requires are very often clothed in a *Hypothesis* which connects with them several superfluous and irrelevant circumstances. Thus the Conception of the Polarization of Light was originally represented under the image of *particles* of light having their poles all turned in the same direction. The Laws of Heat may be made out perhaps most conveniently by conceiving Heat to be a *Fluid*. The Attraction of Gravitation might have been successfully applied to the explanation of facts, if Newton had throughout treated Attraction as the result of an *Ether* diffused through space; a supposition which he has noticed as a possibility. The doctrine of Definite and Multiple Proportions may be conveniently expressed by the hypothesis of *Atoms*. In such cases, the Hypothesis may serve at first to facilitate the introduction of a new Conception. Thus a pervading Ether might for a time remove a difficulty, which some persons find considerable, of imagining a body to exert force at a distance. A Particle with Poles is more easily conceived than Polarization in the abstract. And if hypotheses thus employed will really explain the facts by means of a few simple assumptions, the laws so obtained may afterwards be reduced to a simpler form than that in which they were first suggested. The general laws of Heat, of Attraction, of Polarization, of Multiple Proportions, are now certain, whatever image we may form to ourselves of their ultimate causes.

11. In order, then, to discover scientific truths, suppositions consisting either of new Conceptions, or of new Combinations of old ones, are to be made, till we

find one which succeeds in binding together the Facts. But how are we to find this? How is the trial to be made? What is meant by "success" in these cases? To this we reply, that our inquiry must be, whether the Facts have the same relation in the Hypothesis which they have in reality;—whether the results of our suppositions agree with the phenomena which nature presents to us. For this purpose, we must both carefully observe the phenomena, and steadily trace the consequences of our assumptions, till we can bring the two into comparison. The Conceptions which our hypotheses involve, being derived from certain Fundamental Ideas, afford a basis of rigorous reasoning, as we have shown in the Books respecting those Ideas. And the results to which this reasoning leads, will be susceptible of being verified or contradicted by observation of the facts. Thus the Epicyclical Theory of the Moon, once assumed, determined what the moon's place among the stars ought to be at any given time, and could therefore be tested by actually observing the moon's places. The doctrine that musical strings of the same length, stretched with weights of 1, 4, 9, 16, would give the musical intervals of an octave, a fifth, a fourth, in succession, could be put to the trial by any one whose ear was capable of appreciating those intervals: and the inference which follows from this doctrine by numerical reasoning,—that there must be certain imperfections in the concords of every musical scale,—could in like manner be confirmed by trying various modes of *Temperament*. In like manner all received theories in science, up to the present time, have been established by taking up some supposition, and comparing it, directly or by means of its remoter consequences, with the facts it was intended to embrace. Its agreement, under certain cautions and conditions, of which we may hereafter speak, is held to be the evidence of

its truth. It answers its genuine purpose, the Colligation of Facts.

12. When we have, in any subject, succeeded in one attempt of this kind, and obtained some true Bond of Unity by which the phenomena are held together, the subject is open to further prosecution; which ulterior process may, for the most part, be conducted in a more formal and technical manner. The first great outline of the subject is drawn; and the finishing of the resemblance of nature demands a more minute pencilling, but perhaps requires less of genius in the master. In the pursuance of this task, rules and precepts may be given, and features and leading circumstances pointed out, of which it may often be useful to the inquirer to be aware.

Before proceeding further, I shall speak of some characteristic marks which belong to such scientific processes as are now the subject of our consideration, and which may sometimes aid us in determining when the task has been rightly executed.

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## CHAPTER V.

### OF CERTAIN CHARACTERISTICS OF SCIENTIFIC INDUCTION.

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#### SECT. I.—*Invention a part of Induction.*

1. THE two operations spoken of in the preceding chapters,—the Explication of the Conceptions of our own minds, and the Colligation of observed Facts by the aid of such Conceptions,—are, as we have just said, inseparably connected with each other. When united, and employed in collecting knowledge from the phenomena which the world presents to us, they constitute the mental

process of *Induction* ; which is usually and justly spoken of as the genuine source of all our *real general knowledge* respecting the external world. And we see, from the preceding analysis of this process into its two constituents, from what origin it derives each of its characters. It is *real*, because it arises from the combination of Real Facts, but it is *general*, because it implies the possession of General Ideas. Without the former, it would not be knowledge of the External World ; without the latter, it would not be Knowledge at all. When Ideas and Facts are separated from each other, the neglect of Facts gives rise to empty speculations, idle subtleties, visionary inventions, false opinions concerning the laws of phenomena, disregard of the true aspect of nature : while the want of Ideas leaves the mind overwhelmed, bewildered, and stupefied by particular sensations, with no means of connecting the past with the future, the absent with the present, the example with the rule ; open to the impression of all appearances, but capable of appropriating none. Ideas are the *Form*, facts the *Material*, of our structure. Knowledge does not consist in the empty mould, or in the brute mass of matter, but in the rightly-moulded substance. Induction gathers general truths from particular facts ;—and in her harvest, the corn and the reaper, the solid ears and the binding band, are alike requisite. All our knowledge of nature is obtained by Induction ; the term being understood according to the explanation we have now given. And our knowledge is then most complete, then most truly deserves the name of Science, when both its elements are most perfect ;—when the Ideas which have been concerned in its formation have, at every step, been clear and consistent ;—and when they have, at every step also, been employed in binding together real and certain Facts. Of such Induction, I have already given so many examples and illus-



trations in the two preceding chapters, that I need not now dwell further upon the subject.

2. Induction is familiarly spoken of as the process by which we collect a *General Proposition* from a number of *Particular Cases*: and it appears to be frequently imagined that the general proposition results from a mere juxtaposition of the cases, or at most, from merely conjoining and extending them. But if we consider the process more closely, as exhibited in the cases lately spoken of, we shall perceive that this is an inadequate account of the matter. The particular facts are not merely brought together, but there is a New Element added to the combination by the very act of thought by which they are combined. There is a Conception of the mind introduced in the general proposition, which did not exist in any of the observed facts. When the Greeks, after long observing the motions of the planets, saw that these motions might be rightly considered as produced by the motion of one wheel revolving in the inside of another wheel, these Wheels were Creations of their minds, added to the Facts which they perceived by sense. And even if the wheels were no longer supposed to be material, but were reduced to mere geometrical spheres or circles, they were not the less products of the mind alone,—something additional to the facts observed. The same is the case in all other discoveries. The facts are known, but they are insulated and unconnected, till the discoverer supplies from his own stores a Principle of Connexion. The pearls are there, but they will not hang together till some one provides the String. The distances and periods of the planets were all so many separate facts; by Kepler's Third Law they are connected into a single truth: but the Conceptions which this law involves were supplied by Kepler's mind, and without these, the facts were of no avail. The planets described ellipses round the sun, in

the contemplation of others as well as of Newton; but Newton conceived the deflection from the tangent in these elliptical motions in a new light,—as the effect of a Central Force following a certain law; and then it was, that such a force was discovered truly to exist.

Thus\* in each inference made by Induction, there is introduced some General Conception, which is given, not by the phenomena, but by the mind. The conclusion is not contained in the premises, but includes them by the introduction of a New Generality. In order to obtain our inference, we travel beyond the cases which we have before us; we consider them as mere exemplifications of some Ideal Case in which the relations are complete and intelligible. We take a Standard, and measure the facts by it; and this Standard is constructed by us, not offered by Nature. We assert, for example, that a body left to itself will move on with unaltered velocity; not because our senses ever disclosed to us a body doing this, but because (taking this as our Ideal Case) we find that all actual cases are intelligible and explicable by means of the Conception of *Forces*, causing change and motion, and exerted by surrounding bodies. In like manner, we see bodies striking each other, and thus moving and stopping, accelerating and retarding each other: but in all this, we do not perceive by our senses that abstract quantity, *Momentum*, which is always lost by one body as it is gained by another. This Momentum is a creation of the mind, brought in among the facts, in order to convert their apparent confusion into order, their seeming chance into certainty, their perplexing variety into simplicity. This the Conception of *Momentum gained and lost* does: and in like manner, in any other case in which a truth is established by Induction, some

\* I repeat here remarks made at the end of the *Mechanical Euclid*, p. 178.

Conception is introduced, some Idea is applied, as the means of binding together the facts, and thus producing the truth.

3. Hence in every inference by Induction, there is some Conception *superinduced* upon the Facts: and we may henceforth conceive this to be the peculiar import of the term *Induction*. I am not to be understood as asserting that the term was originally or anciently employed with this notion of its meaning; for the peculiar feature just pointed out in Induction has generally been over-looked. This appears by the accounts generally given of Induction. "Induction," says Aristotle\*, "is when by means of one extreme term† we infer the other extreme term to be true of the middle term." Thus, (to take such exemplifications as belong to our subject,) from knowing that Mercury, Venus, Mars, describe ellipses about the Sun, we infer that all Planets describe ellipses about the Sun. In making this inference syllogistically, we assume that the evident proposition, "Mercury, Venus, Mars, do what all Planets do," may be taken *conversely*, "All Planets do what Mercury, Venus, Mars, do." But we may remark that, in this passage, Aristotle (as was natural in his line of discussion) turns his attention entirely to the *evidence* of the inference; and overlooks a step which is of far more importance to our knowledge, namely, the *invention* of the second extreme term. In the above instance, the particular luminaries, Mercury, Venus, Mars, are one logical *Extreme*; the general designation Planets is the *Middle Term*; but having these before us, how do we come to

\* *Analyt. Prior.*, Lib. II. c. 23. Περὶ τῆς ἐπαγωγῆς.

† The syllogism here alluded to would be this:—

Mercury, Venus, Mars, describe ellipses about the Sun;

All Planets do what Mercury, Venus, Mars, do;

Therefore all Planets describe ellipses about the Sun.

think of *description of ellipses*, which is the other Extreme of the syllogism? When we have once invented this "second Extreme Term," we may, or may not, be satisfied with the evidence of the syllogism; we may, or may not, be convinced that, so far as this property goes, the extremes are co-extensive with the middle term\*; but the *statement* of the syllogism is the important step in science. We know how long Kepler laboured, how hard he fought, how many devices he tried, before he hit upon this *Term*, the Elliptical Motion. He rejected, as we know, many other "second Extreme Terms," for example, various combinations of epicyclical constructions, because they did not represent with sufficient accuracy the special facts of observation. When he had established his premiss, that "Mars does describe an Ellipse about the Sun," he does not hesitate to *guess* at least that, in this respect, he might *convert* the other premiss, and assert that "All the Planets do what Mars does." But the main business was, the inventing and verifying the proposition respecting the Ellipse. The Invention of the Conception was the great step in the *discovery*; the Verification of the Proposition was the great step in the *proof* of the discovery. If Logic consists in pointing out the conditions of proof, the Logic of Induction must consist in showing what are the conditions of proof, in such inferences as this: but this subject must be pursued in the next chapter; I now speak principally of the act of *Invention*, which is requisite in every inductive inference.

4. Although in every inductive inference, an act of invention is requisite, the act soon slips out of notice. Although we bind together facts by superinducing upon them a new Conception, this Conception, once introduced

\* Εἰ οὖν ἀντιστρέφει τὸ Γ τῷ Β καὶ μὴ ὑπερτείνει τὸ μέσον.—  
ARISTOT. *Ibid.*

and applied, is looked upon as inseparably connected with the facts, and necessarily implied in them. Having once had the phenomena bound together in their minds in virtue of the Conception, men can no longer easily restore them back to the detached and incoherent condition in which they were before they were thus combined. The pearls once strung, they seem to form a chain by their nature. Induction has given them a unity which it is so far from costing us an effort to preserve, that it requires an effort to imagine it dissolved. For instance, we usually represent to ourselves the Earth as *round*, the Earth and the Planets as *revolving* about the Sun, and as *drawn* to the Sun by a Central Force; we can hardly understand how it could cost the Greeks, and Copernicus, and Newton, so much pains and trouble to arrive at a view which is to us so familiar. These are no longer to us Conceptions caught hold of and kept hold of by a severe struggle; they are the simplest modes of conceiving the facts: they are really Facts. We are willing to *own* our obligation to those discoverers, but we hardly *feel* it: for in what other manner (we ask in our thoughts,) could we represent the facts to ourselves?

Thus we see why it is that this step of which we now speak, the Invention of a new Conception in every inductive inference, is so generally overlooked that it has hardly been noticed by preceding philosophers. When once performed by the discoverer, it takes a fixed and permanent place in the understanding of every one. It is a thought which, once breathed forth, permeates all men's minds. All fancy they nearly or quite knew it before. It oft was thought, or almost thought, though never till now expressed. Men accept it and retain it, and know it cannot be taken from them, and look upon it as their own. They will not and cannot part with it,

even though they may deem it trivial and obvious. It is a secret, which once uttered, cannot be recalled, even though it be despised by those to whom it is imparted. As soon as the leading term of a new theory has been pronounced and understood, all the phenomena change their aspect. There is a standard to which we cannot help referring them. We cannot fall back into the helpless and bewildered state in which we gazed at them when we possessed no principle which gave them unity. Eclipses arrive in mysterious confusion: the notion of a *Cycle* dispels the mystery. The Planets perform a tangled and mazy dance; but *Epicycles* reduce the maze to order. The Epicycles themselves run into confusion; the conception of an *Ellipse* makes all clear and simple. And thus from stage to stage, new elements of intelligible order are introduced. But this intelligible order is so completely adopted by the human understanding, as to seem part of its texture. Men ask Whether Eclipses follow a Cycle; Whether the Planets describe Ellipses; and they imagine that so long as they do not *answer* such questions rashly, they take nothing for granted. They do not recollect how much they assume in *asking* the question:—how far the conceptions of Cycles and of Ellipses are beyond the visible surface of the celestial phenomena:—how many ages elapsed, how much thought, how much observation, were needed, before men's thoughts were fashioned into the words which they now so familiarly use. And thus they treat the subject, as we have seen Aristotle treating it; as if it were a question, not of invention, but of proof; not of substance, but of form: as if the main thing were not *what* we assert, but *how* we assert it. But for our purpose, it is requisite to bear in mind the feature which we have thus attempted to mark; and to recollect that, in every inference by induction, there is a Conception supplied by the mind and superinduced upon the Facts.

5. In collecting scientific truths by Induction, we often find (as has already been observed), a Definition and a Proposition established at the same time,—introduced together, and mutually dependent on each other. The combination of the two constitutes the Inductive act; and we may consider the Definition as representing the superinduced Conception, and the Proposition as exhibiting the Colligation of Facts.

SECT. II.—*Use of Hypotheses.*

6. To discover a Conception of the mind which will justly represent a train of observed facts is, in some measure, a process of conjecture, as I have stated already; and as I then observed, the business of conjecture is commonly conducted by calling up before our minds several suppositions, and selecting that one which most agrees with what we know of the observed facts. Hence he who has to discover the laws of nature may have to invent many suppositions before he hits upon the right one; and among the endowments which lead to his success, we must reckon that fertility of invention which ministers to him such imaginary schemes, till at last he finds the one which conforms to the true order of nature. A facility in devising hypotheses, therefore, is so far from being a fault in the intellectual character of a discoverer, that it is, in truth, a faculty indispensable to his task. It is, for his purposes, much better that he should be too ready in contriving, too eager in pursuing systems which promise to introduce law and order among a mass of unarranged facts, than that he should be barren of such inventions and hopeless of such success. Accordingly, as we have already noticed, great discoverers have often invented hypotheses which would not answer to all the facts, as well as those which would; and have fancied themselves to have discovered

laws, which a more careful examination of the facts overturned.

The tendencies of our speculative nature\*, carrying us onwards in pursuit of symmetry and rule, and thus producing all true theories, perpetually show their vigour by overshooting the mark. They obtain something, by aiming at much more. They detect the order and connexion which exist, by conceiving imaginary relations of order and connexion which have no existence. Real discoveries are thus mixed with baseless assumptions; profound sagacity is combined with fanciful conjecture; not rarely, or in peculiar instances, but commonly, and in most cases; probably in all, if we could read the thoughts of discoverers as we read the books of Kepler. To try wrong guesses is, with most persons, the only way to hit upon right ones. The character of the true philosopher is, not that he never conjectures hazardously, but that his conjectures are clearly conceived, and brought into rigid contact with facts. He sees and compares distinctly the Ideas and the Things;—the relations of his notions to each other and to phenomena. Under these conditions, it is not only excusable, but necessary for him, to snatch at every semblance of general rule,—to try all promising forms of simplicity and symmetry.

Hence advances in knowledge† are not commonly made without the previous exercise of some boldness and license in guessing. The discovery of new truths requires, undoubtedly, minds careful and scrupulous in

\* I here take the liberty of characterizing inventive minds in general in the same phraseology which, in the *History of Science*, I have employed in reference to particular examples. These expressions are what I have used in speaking of the discoveries of Copernicus.—*Hist. Ind. Sci.*, B. v. c. ii.

† These observations are made on occasion of Kepler's speculations, and are illustrated by reference to his discoveries.—*Hist. Ind. Sci.*, B. v. c. iv. sect. 1.



examining what is suggested; but it requires, no less, such as are quick and fertile in suggesting. What is Invention, except the talent of rapidly calling before us the many possibilities, and selecting the appropriate one? It is true, that when we have rejected all the inadmissible suppositions, they are often quickly forgotten; and few think it necessary to dwell on these discarded hypotheses, and on the process by which they were condemned. But all who discover truths, must have reasoned upon many errors to obtain each truth; every accepted doctrine must have been one chosen out of many candidates. If many of the guesses of philosophers of bygone times now appear fanciful and absurd, because time and observation have refuted them, others, which were at the time equally gratuitous, have been confirmed in a manner which makes them appear marvellously sagacious. To form hypotheses, and then to employ much labour and skill in refuting, if they do not succeed in establishing them, is a part of the usual process of inventive minds. Such a proceeding belongs to the *rule* of the genius of discovery, rather than (as has often been taught in modern times) to the *exception*.

7. But if it be an advantage for the discoverer of truth that he be ingenious and fertile in inventing hypotheses which may connect the phenomena of nature, it is indispensably requisite that he be diligent and careful in comparing his hypotheses with the facts, and ready to abandon his invention as soon as it appears that it does not agree with the course of actual occurrences. This constant comparison of his own conceptions and supposition with observed facts under all aspects, forms the leading employment of the discoverer: this candid and simple love of truth, which makes him willing to suppress the most favourite production of his own ingenuity as soon as it appears to be at variance with realities,

constitutes the first characteristic of his temper. He must have neither the blindness which cannot, nor the obstinacy which will not, perceive the discrepancy of his fancies and his facts. He must allow no indolence, or partial views, or self-complacency, or delight in seeming demonstration, to make him tenacious of the schemes which he devises, any further than they are confirmed by their accordance with nature. The framing of hypotheses is, for the inquirer after truth, not the end, but the beginning of his work. Each of his systems is invented, not that he may admire it and follow it into all its consistent consequences, but that he may make it the occasion of a course of active experiment and observation. And if the results of this process contradict his fundamental assumptions, however ingenious, however symmetrical, however elegant his system may be, he rejects it without hesitation. He allows no natural yearning for the offspring of his own mind to draw him aside from the higher duty of loyalty to his sovereign, Truth: to her he not only gives his affections and his wishes, but strenuous labour and scrupulous minuteness of attention.

We may refer to what we have said of Kepler, Newton, and other eminent philosophers, for illustrations of this character. In Kepler we have remarked\* the courage and perseverance with which he undertook and executed the task of computing his own hypotheses: and, as a still more admirable characteristic, that he never allowed the labour he had spent upon any conjecture to produce any reluctance in abandoning the hypothesis, as soon as he had evidence of its inaccuracy. And in the history of Newton's discovery that the moon is retained in her orbit by the force of gravity, we have noticed the same moderation in maintaining the hypothesis, after it had once occurred to the author's mind.

\* *Hist. Ind. Sci.*, B. v. c. iv. sect. 1.

The hypothesis required that the moon should fall from the tangent of her orbit every second through a space of sixteen feet; but according to his first calculations it appeared that in fact she only fell through a space of thirteen feet in that time. The difference seems small, the approximation encouraging, the theory plausible; a man in love with his own fancies would readily have discovered or invented some probable cause of the difference. But Newton acquiesced in it as a disproof of his conjecture, and "laid aside at that time any further thoughts of this matter\*."

8. It has often happened that those who have undertaken to instruct mankind have not possessed this pure love of truth and comparative indifference to the maintenance of their own inventions. Men have frequently adhered with great tenacity and vehemence to the hypotheses which they have once framed; and in their affection for these, have been prone to overlook, to distort, and to misinterpret facts. In this manner, *Hypotheses* have so often been prejudicial to the genuine pursuit of truth, that they have fallen into a kind of obloquy; and have been considered as dangerous temptations and fallacious guides. Many warnings have been uttered against the fabrication of hypotheses by those who profess to teach philosophy; many disclaimers of such a course by those who cultivate science.

Thus we shall find Bacon frequently discommending this habit, under the name of "anticipation of the mind," and Newton thinks it necessary to say emphatically "hypotheses non fingo." It has been constantly urged that the inductions by which sciences are formed must be *cautious* and *rigorous*; and the various imaginations which passed through Kepler's brain, and to which he has given utterance, have been blamed or pitied as la-

\* *Hist. Ind. Sci.*, B. VII. c. ii. sect. 3.

mentable instances of an unphilosophical frame of mind. Yet it has appeared in the preceding remarks that hypotheses rightly used are among the helps, far more than the dangers, of science ;—that scientific induction is not a “cautious” or a “rigorous” process in the sense of *abstaining from* such suppositions, but in *not adhering to* them till they are confirmed by fact, and in carefully seeking from facts confirmation or refutation. Kepler’s character was, not that he was peculiarly given to the construction of hypotheses, but that he narrated with extraordinary copiousness and candour the course of his thoughts, his labours, and his feelings. In the minds of most persons, as we have said, the inadmissible suppositions, when rejected, are soon forgotten : and thus the trace of them vanishes from the thoughts, and the successful hypothesis alone holds its place in our memory. But in reality, many other transient suppositions must have been made by all discoverers ;—hypotheses which are not afterwards asserted as true systems, but entertained for an instant ;—“tentative hypotheses,” as they have been called. Each of these hypotheses is followed by its corresponding train of observations, from which it derives its power of leading to truth. The hypothesis is like the captain, and the observations like the soldiers of an army : while he appears to command them, and in this way to work his own will, he does in fact derive all his power of conquest from their obedience, and becomes helpless and useless if they mutiny.

Since the discoverer has thus constantly to work his way onwards by means of hypotheses, false and true, it is highly important for him to possess talents and means for rapidly *testing* each supposition as it offers itself. In this as in other parts of the work of discovery, success has in general been mainly owing to the native ingenuity and sagacity of the discoverer’s mind. Yet

some Rules tending to further this object have been delivered by eminent philosophers, and some others may perhaps be suggested. Of these we shall here notice only some of the most general, leaving for a future chapter the consideration of some more limited and detailed processes by which, in certain cases, the discovery of the laws of nature may be materially assisted.

### SECT. III.—*Tests of Hypotheses.*

9. A Maxim which it may be useful to recollect is this;—that *hypotheses may often be of service to science, when they involve a certain portion of incompleteness, and even of error.* The object of such inventions is to bind together facts which without them are loose and detached; and if they do this, they may lead the way to a perception of the true rule by which the phenomena are associated together, even if they themselves somewhat misstate the matter. The imagined arrangement enables us to contemplate, as a whole, a collection of special cases which perplex and overload our minds when they are considered in succession; and if our scheme has so much of truth in it as to conjoin what is really connected, we may afterwards duly correct or limit the mechanism of this connexion. If our hypothesis renders a reason for the agreement of cases really similar, we may afterwards find this reason to be false, but we shall be able to translate it into the language of truth.

A conspicuous example of such an hypothesis, one which was of the highest value to science, though very incomplete, and as a representation of nature altogether false, is seen in the *Doctrine of epicycles* by which the ancient astronomers explained the motions of the sun, moon, and planets. This doctrine connected the places and velocities of these bodies at particular times in a

manner which was, in its general features, agreeable to nature. Yet this doctrine was erroneous in its assertion of the circular nature of all the celestial motions, and in making the heavenly bodies revolve round the earth. It was, however, of immense value to the progress of astronomical science; for it enabled men to express and reason upon many important truths which they discovered respecting the motion of the stars, up to the time of Kepler. Indeed we can hardly imagine that astronomy could, in its outset, have made so great a progress under any other form, as it did in consequence of being cultivated in this shape of the incomplete and false epicyclical hypothesis.

We may notice another instance of an exploded hypothesis, which is generally mentioned only to be ridiculed, and which undoubtedly is both false in the extent of its assertion, and unphilosophical in its expression; but which still, in its day, was not without merit. I mean the doctrine of *Nature's horror of a vacuum* (*fuga vacui*), by which the action of siphons and pumps and many other phenomena were explained, till Mersenne and Pascal taught a truer doctrine. This hypothesis was of real service; for it brought together many facts which really belong to the same class, although they are very different in their first aspect. A scientific writer of modern times\* appears to wonder that men did not at once divine the weight of the air from which the phenomena formerly ascribed to the *fuga vacui* really result. "Loaded, compressed by the atmosphere," he says, "they did not recognize its action. In vain all nature testified that air was elastic and heavy; they shut their eyes to her testimony. The water rose in pumps and flowed in siphons at that time, as it does at this day. They could not separate the boards of a pair of bellows of which the holes were

\* Deluc, *Modifications de l'Atmosphere*, Partie 1.

stopped; and they could not bring together the same boards without difficulty, if they were at first separated. Infants sucked the milk of their mothers; air entered rapidly into the lungs of animals at every inspiration; cupping-glasses produced tumours on the skin; and in spite of all the striking proofs of the weight and elasticity of the air, the ancient philosophers maintained resolutely that air was light, and explained all these phenomena by the horror which they said nature had for a vacuum." It is curious that it should not have occurred to the author while writing this, that if these facts, so numerous and various, can all be accounted for by *one* principle, there is a strong presumption that the principle is not altogether baseless. And in reality is it not true that nature *does* abhor a vacuum, and do all she can to avoid it? No doubt this power is not unlimited; and we can trace it to a mechanical cause, the pressure of the circumambient air. But the tendency, arising from this pressure, which the bodies surrounding a space void of air have to rush into it, may be expressed, in no extravagant or unintelligible manner, by saying that nature has a repugnance to a vacuum.

That imperfect and false hypotheses, though they may thus explain *some* phenomena, and may be useful in the progress of science, cannot explain *all* phenomena;—and that we are never to rest in our labours or acquiesce in our results, till we have found some view of the subject which *is* consistent with *all* the observed facts:—will of course be understood. We shall afterwards have to speak of the other steps of such a progress.

10. The hypotheses which we accept ought to explain phenomena which we have observed. But they ought to do more than this: our hypotheses ought to *foretel* phenomena which have not yet been observed;—at least all of the same kind as those which the hypothesis was

invented to explain. For our assent to the hypothesis implies that it is held to be true of all particular instances. That these cases belong to past or to future times, that they have or have not already occurred, makes no difference in the applicability of the rule to them. Because the rule prevails, it includes all cases; and will determine them all, if we can only calculate its real consequences. Hence it will predict the results of new combinations, as well as explain the appearances which have occurred in old ones. And that it does this with certainty and correctness, is one mode in which the hypothesis is to be verified as right and useful.

The scientific doctrines which have at various periods been established have been verified in this manner. For example, the *Epicyclical Theory* of the heavens was confirmed by its *predicting* truly eclipses of the sun and moon, configurations of the planets, and other celestial phenomena; and by its leading to the construction of Tables by which the places of the heavenly bodies were given at every moment of time. The truth and accuracy of these predictions were a proof that the hypothesis was valuable and, at least to a great extent, true; although, as was afterwards found, it involved a false representation of the structure of the heavens. In like manner, the discovery of the *Laws of Refraction* enabled mathematicians to *predict*, by calculation, what would be the effect of any new form or combination of transparent lenses. Newton's hypothesis of *Fits of Easy Transmission and Easy Reflection* in the particles of light, although not confirmed by other kinds of facts, involved a true statement of the law of the phenomena which it was framed to include, and served to *predict* the forms and colours of thin plates for a wide range of given cases. The hypothesis that Light operates by *Undulations* and



*Interferences*, afforded the means of *predicting* results under a still larger extent of conditions. In like manner in the progress of chemical knowledge, the doctrine of *Phlogiston* supplied the means of *foreseeing* the consequence of many combinations of elements, even before they were tried; but the *Oxygen Theory*, besides affording predictions, at least equally exact, with regard to the general results of chemical operations, included all the facts concerning the relations of weight of the elements and their compounds, and enabled chemists to *foresee* such facts in untried cases. And the Theory of *Electromagnetic Forces*, as soon as it was rightly understood, enabled those who had mastered it to *predict* motions such as had not been before observed, which were accordingly found to take place.

Men cannot help believing that the laws laid down by discoverers must be in a great measure identical with the real laws of nature, when the discoverers thus determine effects beforehand in the same manner in which nature herself determines them when the occasion occurs. Those who can do this, must, to a considerable extent, have detected nature's secret;—must have fixed upon the conditions to which she attends, and must have seized the rules by which she applies them. Such a coincidence of untried facts with speculative assertions cannot be the work of chance, but implies some large portion of truth in the principles on which the reasoning is founded. To trace order and law in that which has been observed, may be considered as interpreting what nature has written down for us, and will commonly prove that we understand her alphabet. But to predict what has not been observed, is to attempt ourselves to use the legislative phrases of nature; and when she responds plainly and precisely to that which we thus utter, we cannot but suppose that we have in a great measure made ourselves masters of the

meaning and structure of her language. The prediction of results, even of the same kind as those which have been observed, in new cases, is a proof of real success in our inductive processes.

11. We have here spoken of the prediction of facts *of the same kind* as those from which our rule was collected. But the evidence in favour of our induction is of a much higher and more forcible character when it enables us to explain and determine cases of a *kind different* from those which were contemplated in the formation of our hypothesis. The instances in which this has occurred, indeed, impress us with a conviction that the truth of our hypothesis is certain. No accident could give rise to such an extraordinary coincidence. No false supposition could, after being adjusted to one class of phenomena, exactly represent a different class, when the agreement was unforeseen and un contemplated. That rules springing from remote and unconnected quarters should thus leap to the same point, can only arise from *that* being the point where truth resides.

Accordingly the cases in which inductions from classes of facts altogether different have thus *jumped together*, belong only to the best established theories which the history of science contains. And as I shall have occasion to refer to this peculiar feature in their evidence, I will take the liberty of describing it by a particular phrase; and will term it the *Consilience of Inductions*.

It is exemplified principally in some of the greatest discoveries. Thus it was found by Newton that the doctrine of the Attraction of the Sun varying according to the Inverse Square of this distance, which explained Kepler's *Third Law* of the proportionality of the cubes of the distances to the squares of the periodic times of the planets, explained also his *First* and *Second Laws* of the elliptical motion of each planet; although no connexion

of these laws had been visible before. Again, it appeared that the force of Universal Gravitation, which had been inferred from the *Perturbations* of the moon and planets by the sun and by each other, also accounted for the fact, apparently altogether dissimilar and remote, of the *Precession of the equinoxes*. Here was a most striking and surprising coincidence, which gave to the theory a stamp of truth beyond the power of ingenuity to counterfeit. In like manner in Optics; the hypothesis of alternate Fits of easy Transmission and Reflection would explain the colours of thin plates, and indeed was devised and adjusted for that very purpose; but it could give no account of the phenomena of the fringes of shadows. But the doctrine of Interferences, constructed at first with reference to phenomena of the nature of the *Fringes*, explained also the *Colours of thin plates* better than the supposition of the fits invented for that very purpose. And we have in Physical Optics another example of the same kind, which is quite as striking as the explanation of precession by inferences from the facts of perturbation. The doctrine of Undulations propagated in a Spheroidal Form was contrived at first by Huyghens, with a view to explain the laws of *Double Refraction* in calc-spar; and was pursued with the same view by Fresnel. But in the course of the investigation it appeared, in a most unexpected and wonderful manner, that this same doctrine of spheroidal undulations, when it was so modified as to account for the directions of the two refracted rays, accounted also for the positions of their *Planes of Polarization\**; a phenomenon which, taken by itself, it had perplexed previous mathematicians, even to represent.

The Theory of Universal Gravitation, and of the Undulatory Theory of Light, are, indeed, full of examples of this Consilience of Inductions. With regard to the

\* *Hist. Ind. Sci.*, B. IX. c. xi. sect. 4.

latter, it has been justly asserted by Herschel, that the history of the undulatory theory was a succession of *felicities*\*. And it is precisely the unexpected coincidences of results drawn from distant parts of the subject which are properly thus described. Thus the Laws of the *Modification of polarization* to which Fresnel was led by his general views, accounted for the Rule respecting the *Angle at which light is polarized*, discovered by Sir D. Brewster†. The conceptions of the theory pointed out peculiar *Modifications* of the phenomena when *Newton's rings* were produced by polarized light, which modifications were ascertained to take place in fact, by Arago and Airy‡. When the beautiful phenomena of *Dipolarized light* were discovered by Arago and Biot, Young was able to declare that they were reducible to the general laws of *Interference* which he had already established§. And what was no less striking a confirmation of the truth of the theory, *Measures* of the same element deduced from various classes of facts were found to coincide. Thus the *Length* of a luminiferous undulation, calculated by Young from the measurement of *Fringes* of shadows, was found to agree very nearly with the previous calculation from the colours of *Thin plates*||.

No example can be pointed out, in the whole history of science, so far as I am aware, in which this Consilience of Inductions has given testimony in favour of an hypothesis afterwards discovered to be false. If we take one class of facts only, knowing the law which they follow, we may construct an hypothesis, or perhaps several, which may represent them: and as new circumstances are discovered, we may often adjust the hypothesis so as to correspond to these also. But when the hypothesis, of itself and without adjustment for the pur-

\* See *Hist. Ind. Sci.*, B. ix. c. xii.

† *Ib.*, c. xi. sect. 4.

‡ *Ib.*, c. xiii. sect. 6. § *Ib.*, c. xi. sect. 5.

|| *Ib.*, c. xi. sect. 2.

pose, gives us the rule and reason of a class of facts not contemplated in its construction, we have a criterion of its reality, which has never yet been produced in favour of falsehood.

12. In the preceding Article I have spoken of the hypothesis with which we compare our facts as being framed *all at once*, each of its parts being included in the original scheme. In reality, however, it often happens that the various suppositions which our system contains are *added* upon occasion of different researches. Thus in the Ptolemaic doctrine of the heavens, new epicycles and eccentrics were added as new inequalities of the motions of the heavenly bodies were discovered; and in the Newtonian doctrine of material rays of light, the supposition that these rays had "fits," was added to explain the colours of thin plates; and the supposition that they had "sides" was introduced on occasion of the phenomena of polarization. In like manner other theories have been built up of parts devised at different times.

This being the mode in which theories are often framed, we have to notice a distinction which is found to prevail in the progress of true and of false theories. In the former class all the additional suppositions *tend to simplicity* and harmony; the new suppositions resolve themselves into the old ones, or at least require only some easy modification of the hypothesis first assumed: the system becomes more coherent as it is further extended. The elements which we require for explaining a new class of facts are already contained in our *system*. Different members of the theory run together, and we have thus a constant convergence to unity. In false theories, the contrary is the case. The new suppositions are something altogether additional;—not suggested by the original scheme; perhaps difficult to reconcile with it. Every such addition adds to the complexity of the

hypothetical system, which at last becomes unmanageable, and is compelled to surrender its place to some simpler explanation.

Such a false theory, for example, was the ancient doctrine of eccentrics and epicycles. It explained the general succession of the Places of the Sun, Moon, and Planets; it would not have explained the proportion of their Magnitudes at different times, if these could have been accurately observed; but this the ancient astronomers were unable to do. When, however, Tycho and other astronomers came to be able to observe the planets accurately in all positions, it was found that *no* combination of *equable* circular motions would exactly represent all the observations. We may see, in Kepler's works, the many new modifications of the epicyclical hypothesis which offered themselves to him; some of which would have agreed with the phenomena with a certain degree of accuracy, but not so great a degree as Kepler, fortunately for the progress of science, insisted upon obtaining. After these epicycles had been thus accumulated, they all disappeared and gave way to the simpler conception of an *elliptical* motion. In like manner, the discovery of new inequalities in the Moon's motions encumbered her system more and more with new machinery, which was at last rejected all at once in favour of the *elliptical* theory. Astronomers could not but suppose themselves in a wrong path, when the prospect grew darker and more entangled at every step.

Again; the Cartesian system of Vortices might be said to explain the primary phenomena of the revolutions of planets about the sun, and satellites about planets. But the elliptical form of the orbits required new suppositions. Bernoulli ascribed this curve to the *shape* of the planet, operating on the stream of the vortex in a manner similar to the rudder of a boat. But

then the motions of the aphelia, and of the nodes,—the perturbations,—even the action of gravity towards the earth,—could not be accounted for without new and independent suppositions. Here was none of the simplicity of truth. The theory of Gravitation, on the other hand, became more simple as the facts to be explained became more numerous. The attraction of the sun accounted for the motions of the planets; the attraction of the planets was the cause of the motion of the satellites. But this being assumed, the perturbations, the motions of the nodes and aphelia, only made it requisite to extend the attraction of the sun to the satellites, and that of the planets to each other:—the tides, the spheroidal form of the earth, the precession, still required nothing more than that the moon and sun should attract the parts of the earth, and that these should attract each other;—so that all the suppositions resolved themselves into the single one, of the universal gravitation of all matter. It is difficult to imagine a more convincing manifestation of simplicity and unity.

Again, to take an example from another science;—the doctrine of Phlogiston brought together many facts in a very plausible manner,—combustion, acidification, and others,—and very naturally prevailed for a while. But the balance came to be used in chemical operations, and the facts of weight as well as of combination were to be accounted for. On the phlogistic theory, it appeared that this could not be done without a new supposition, and *that*, a very strange one;—that phlogiston was an element not only not heavy, but absolutely light, so that it diminished the weight of the compounds into which it entered. Some chemists for a time adopted this extravagant view; but the wiser of them saw, in the necessity of such a supposition to the defence of the theory, an evidence that the hypothesis of

an element *phlogiston* was erroneous. And the opposite hypothesis, which taught that oxygen was subtracted, and not phlogiston added, was accepted because it required no such novel and inadmissible assumption.

Again, we find the same evidence of truth in the progress of the Undulatory Theory of light, in the course of its application from one class of facts to another. Thus we explain Reflection and Refraction by undulations; when we come to Thin Plates, the requisite "fits" are already involved in our fundamental hypothesis, for they are the length of an undulation: the phenomena of Diffraction also require such intervals; and the intervals thus required agree exactly with the others in magnitude, so that no new property is needed. Polarization for a moment appears to require some new hypothesis; yet this is hardly the case; for the direction of our vibrations is hitherto arbitrary:—we allow polarization to decide it, and we suppose the undulations to be transverse. Having done this for the sake of Polarization, we turn to the phenomena of Double Refraction, and inquire what new hypothesis they require. But the answer is, that they require none: the supposition of transverse vibrations, which we have made in order to explain Polarization, gives us also the law of Double Refraction. Truth may give rise to such a coincidence; falsehood cannot. Again, the facts of Dipolarization come into view. But they hardly require any new assumption; for the difference of optical elasticity of crystals in different directions, which is already assumed in uniaxial crystals\*, is extended to biaxial exactly according to the law of symmetry; and this being done, the laws of the phenomena, curious and complex as they are, are fully explained. The phenomena of Circular Polarization by internal reflection, instead of requiring a new hypothesis,

\* *Hist. Ind. Sci.*, B. ix. c. xi. sect. 5.



are found to be given by an interpretation of an apparently inexplicable result of an old hypothesis. The Circular Polarization of Quartz and its Double Refraction does indeed appear to require a new assumption, but still not one which at all disturbs the form of the theory; and in short, the whole history of this theory is a progress, constant and steady, often striking and startling, from one degree of evidence and consistence to another of higher order.

In the Emission Theory, on the other hand, as in the theory of solid epicycles, we see what we may consider as the natural course of things in the career of a false theory. Such a theory may, to a certain extent, explain the phenomena which it was at first contrived to meet; but every new class of facts requires a new supposition—an addition to the machinery: and as observation goes on, these incoherent appendages accumulate, till they overwhelm and upset the original frame-work. Such has been the hypothesis of the Material Emission of light. In its original form, it explained Reflection and Refraction: but the colours of Thin Plates added to it the Fits of easy Transmission and Reflection; the phenomena of Diffraction further invested the emitted particles with complex laws of Attraction and Repulsion; Polarization gave them Sides: Double Refraction subjected them to peculiar Forces emanating from the axes of the crystal: finally, Dipolarization loaded them with the complex and unconnected contrivance of Moveable Polarization: and even when all this had been done, additional mechanism was wanting. There is here no unexpected success, no happy coincidence, no convergence of principles from remote quarters. The philosopher builds the machine, but its parts do not fit. They hold together only while he presses them. This is not the character of truth.

As another example of the application of the Maxim now under consideration, I may perhaps be allowed to refer to the judgment which, in the History of Thermotics, I have ventured to give respecting Laplace's Theory of Gases. I have stated\*, that we cannot help forming an unfavourable judgment of this theory, by looking for that great characteristic of true theory; namely, that the hypotheses which were assumed to account for *one class* of facts are found to explain *another class* of a different nature. Thus Laplace's first suppositions explain the connexion of Compression with Density, (the law of Boyle and Mariotte,) and the connexion of Elasticity with Heat, (the law of Dalton and Gay Lussac.) But the theory requires other assumptions when we come to Latent Heat; and yet these new assumptions produce no effect upon the calculations in any application of the theory. When the hypothesis, constructed with reference to the Elasticity and Temperature, is applied to another class of facts, those of Latent Heat, we have no Simplification of the Hypothesis, and therefore no evidence of the truth of the theory.

13. The two last sections of this chapter direct our attention to two circumstances, which tend to prove, in a manner which we may term irresistible, the truth of the theories which they characterize:—the *Consilience of Inductions* from different and separate classes of facts;—and the progressive *Simplification of the Theory* as it is extended to new cases. These two Characters are, in fact, hardly different; they are exemplified by the same cases. For if these Inductions, collected from one class of facts, supply an unexpected explanation of a new class, which is the case first spoken of, there will be no need for new machinery in the hypothesis to apply it to the newly-contemplated facts; and thus, we have a case in

\* *Hist. Ind. Sci.*, B. x. c. iv.

which the system does not become more complex when its application is extended to a wider field, which was the character of true theory in its second aspect. The Consiliences of our Inductions give rise to a constant Convergence of our Theory towards Simplicity and Unity.

But, moreover, both these cases of the extension of the theory, without difficulty or new suppositions, to a wider range and to new classes of phenomena, may be conveniently considered in yet another point of view; namely, as successive steps by which we gradually ascend in our speculative views to a higher and higher point of generality. For when the theory, either by the concurrence of two indications, or by an extension without complication, has included a new range of phenomena, we have, in fact, a new induction of a more general kind, to which the inductions formerly obtained are subordinate, as particular cases to a general proposition. We have in such examples, in short, an instance of *successive generalization*. This is a subject of great importance, and deserving of being well illustrated; it will come under our notice in the next chapter.

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## CHAPTER VI.

### OF THE LOGIC OF INDUCTION.

1. THE subject to which the present chapter refers is described by phrases which are at the present day familiarly used in speaking of the progress of knowledge. We hear very frequent mention of *ascending from particular to general* propositions, and from these to propositions still more general;—of truths *included* in other truths of a higher degree of generality;—of different *stages of generalization*;—and of the *highest step* of the

process of discovery, to which all others are subordinate and preparatory. As these expressions, so familiar to our ears, especially since the time of Francis Bacon, denote, very significantly, processes and relations which are of great importance in the formation of science, it is necessary for us to give a clear account of them, illustrated with general exemplifications; and this we shall endeavour to do.

We have, indeed, already explained that science consists of propositions which include the facts from which they were collected; and other wider propositions, collected in like manner from the former, and including them. Thus, that the stars, the moon, the sun, rise, culminate, and set, are facts *included* in the proposition that the heavens, carrying with them all the celestial bodies, have a diurnal revolution about the axis of the earth. Again, the observed monthly motions of the moon, and the annual motions of the sun, are *included* in certain propositions concerning the movements of those luminaries with respect to the stars. But all these propositions are really *included* in the doctrine that the earth, revolving on its axis, moves round the sun, and the moon round the earth. These movements, again, considered as facts, are explained and *included* in the statement of the forces which the earth exerts upon the moon, and the sun upon the earth. Again, this doctrine of the forces of these two bodies is *included* in the assertion, that all the bodies of the solar system, and all parts of matter, exert forces, each upon each. And we might easily show that all the leading facts in astronomy are comprehended in the same generalization. In like manner with regard to any other science, so far as its truths have been well established and fully developed, we might show that it consists of a gradation of propositions, proceeding from the most special facts to the most general

theoretical assertions. We shall exhibit this gradation in some of the principal branches of science.

2. This gradation of truths, successively included in other truths, may be conveniently represented by *Tables* resembling the genealogical tables by which the derivation of descendants from a common ancestor is exhibited; except that it is proper in this case to invert the form of the Table, and to make it converge to unity downwards instead of upwards, since it has for its purpose to express, not the derivation of many from one, but the collection of one truth from many things. Two or more co-ordinate facts or propositions may be ranged side by side, and joined by some mark of connexion, (a bracket, as  or ,) beneath which may be placed the more general proposition which is collected by induction from the former. Again, propositions co-ordinate with this more general one may be placed on a level with it; and the combination of these, and the result of the combination, may be indicated by brackets in the same manner; and so on, through any number of gradations. By this means the streams of knowledge from various classes of facts will constantly run together into a smaller and smaller number of channels; like the confluent rivulets of a great river, coming together from many sources, uniting their ramifications so as to form larger branches, these again uniting in a single trunk. The *genealogical tree* of each great portion of science, thus formed, will contain all the leading truths of the science arranged in their due co-ordination and subordination. Such Tables, constructed for the sciences of Astronomy and of Optics, will be given at the end of this chapter.

3. The union of co-ordinate propositions into a proposition of a higher order, which occurs in this Tree of Science wherever two twigs unite in one branch, is, in each case, an example of *Induction*. The single propo-

sition is collected by the process of induction from its several members. But here we may observe, that the image of a mere *union* of the parts at each of these points, which the figure of a tree or a river presents, is very inadequate to convey the true state of the case; for in Induction, as we have seen, besides mere collection of particulars, there is always a *new conception*, a principle of connexion and unity, supplied by the mind, and super-induced upon the particulars. There is not merely a juxta-position of materials, by which the new proposition contains all that its component parts contained; but also a formative act exerted by the understanding, so that these materials are contained in a new shape. We must remember, therefore, that our Inductive Tables, although they represent the elements and the order of these inductive steps, do not fully represent the whole signification of the process in each case.

4. The principal features of the progress of science spoken of in the last chapter are clearly exhibited in these Tables; namely, the *Consilience of Inductions*, and the constant Tendency to Simplicity observable in true theories. Indeed in all cases in which from propositions of considerable generality, propositions of a still higher degree are obtained, there is a convergence of inductions; and if in one of the lines which thus converge, the steps be rapidly and suddenly made in order to meet the other line, we may consider that we have an example of Consilience. Thus when Newton had collected from Kepler's Laws the Central Force of the sun, and from these, combined with other facts, the Universal Force of all the heavenly bodies, he suddenly turned round to include in his generalization the Precession of the Equinoxes, which he declared to arise from the attraction of the sun and moon upon the protuberant part of the terrestrial spheroid. The apparent remoteness of this fact, in its nature, from the others with which he thus asso-

ciated it, causes this part of his reasoning to strike us as a remarkable example of *Consilience*. Accordingly, in the Table of Astronomy we find that the columns which contain the facts and theories relative to the *sun* and *planets*, after exhibiting several stages of induction within themselves, are at length suddenly connected with a column till then quite distinct, containing the *precession of the equinoxes*. In like manner, in the Table of Optics, the columns which contain the facts and theories relative to *double refraction*, and those which include *polarization by crystals*, each go separately through several stages of induction; and then these two sets of columns are suddenly connected by Fresnel's mathematical induction that double refraction and polarization arise from the same cause: thus exhibiting a remarkable *Consilience*.

5. The constant *Tendency to Simplicity* in the sciences of which the progress is thus represented, appears from the form of the Table itself; for the single trunk into which all the branches converge, contains in itself the substance of all the propositions by means of which this last generalization was arrived at. It is true, that this ultimate result is sometimes not so simple as in the Table it appears: for instance, the ultimate generalization of the Table exhibiting the progress of Physical Optics,—namely, that Light consists in Undulations,—must be understood as including some other hypotheses; as, that the undulations are transverse, that the ether through which they are propagated has its elasticity in crystals and other transparent bodies regulated by certain laws; and the like. Yet still, even acknowledging all the complication thus implied, the Table in question evidences clearly enough the constant advance towards unity, consistency, and simplicity, which have marked the progress of this Theory. The same is the case in the Inductive Table of Astronomy in a still greater degree.

6. These Tables naturally afford the opportunity of

assigning to each of the distinct steps of which the progress of science consists, the name of the *Discoverer* to whom it is due. Every one of the inductive processes which the brackets of our Tables mark, directs our attention to some person by whom the induction was first distinctly made. These names I have endeavoured to put in their due places in the Tables; and the Inductive Tree of our knowledge in each science becomes, in this way, an exhibition of the claims of each discoverer to distinction, and, as it were, a Genealogical Tree of scientific nobility. It is by no means pretended that such a tree includes the names of all the meritorious labourers in each department of science. Many persons are most usefully employed in collecting and verifying truths, who do not advance to any new truths. The labours of a number of such are included in each stage of our ascent. But such Tables as we have now before us will present to us the names of all the most eminent discoverers: for the main steps of which the progress of science consists, are transitions from more particular to more general truths, and must therefore be rightly given by these Tables; and those must be the greatest names in science to whom the principal events of its advance are thus due.

7. The Tables, as we have presented them, exhibit the course by which we pass from particular to general through various gradations, and so to the most general. They display the order of *discovery*. But by reading them in an inverted manner, beginning at the single comprehensive truths with which the Tables end, and tracing these back into the more partial truths, and these again into special facts, they answer another purpose;—they exhibit the process of *verification* of discoveries once made. For each of our general propositions is true in virtue of the truth of the narrower propositions which it



involves; and we cannot satisfy ourselves of its truth in any other way than by ascertaining that these its constituent elements are true. To assure ourselves that the sun attracts the planets with forces varying inversely as the square of the distance, we must analyze by geometry the motion in an ellipse about the focus, so as to see that it does imply such a force. We must also verify those calculations by which the observed places of each planet are stated to be included in an ellipse. These calculations involve assumptions respecting the path which the earth describes about the sun, which assumptions must again be verified by reference to observation. And thus, proceeding from step to step, we resolve the most general truths into their constituent parts; and these again into their parts; and by testing, at each step, both the reality of the asserted ingredients and the propriety of the conjunction, we establish the whole system of truths, however wide and various it may be.

8. It is a very great advantage, in such a mode of exhibiting scientific truths, that it resolves the verification of the most complex and comprehensive theories, into a number of small steps, of which almost any one falls within the reach of common talents and industry. That *if* the particulars of any one step be true, the generalization also is true, any person with a mind properly disciplined may satisfy himself by a little study. That each of these particular propositions *is* true, may be ascertained, by the same kind of attention, when this proposition is resolved into *its* constituent and more special propositions. And thus we may proceed, till the most general truth is broken up into small and manageable portions. Of these portions, each may appear by itself narrow and easy; and yet they are so woven together, by hypothesis and conjunction, that the truth of the parts necessarily assures us of the truth of the whole.

The verification is of the same nature as the verification of a large and complex statement of great sums received by a mercantile office on various accounts from many quarters. The statement is separated into certain comprehensive heads, and these into others less extensive; and these again into smaller collections of separate articles, each of which can be inquired into and reported on by separate persons. And thus at last, the mere addition of numbers performed by these various persons, and the summation of the results which they obtain, executed by other accountants, is a complete and entire security that there is no error in the whole of the process.

9. This comparison of the process by which we verify scientific truth to the process of Book-keeping in a large commercial establishment, may appear to some persons not sufficiently dignified for the subject. But, in fact, the possibility of giving this formal and business-like aspect to the evidence of science, as involved in the process of successive generalization, is an inestimable advantage. For if no one could pronounce concerning a wide and profound theory except he who could at once embrace in his mind the whole range of inference, extending from the special facts up to the most general principles, none but the greatest geniuses would be entitled to judge concerning the truth or error of scientific discoveries. But, in reality, we seldom need to verify more than one or two steps of such discoveries at one time; and this may commonly be done (when the discoveries have been fully established and developed,) by any one who brings to the task clear conceptions and steady attention. The progress of science is gradual: the discoveries which are successively made, are also verified successively. We have never any very large collections of them on our hands at once. The doubts

and uncertainties of any one who has studied science with care and perseverance are generally confined to a few points. If he can satisfy himself upon these, he has no misgivings respecting the rest of the structure; which has indeed been repeatedly verified by other persons in like manner. The fact that science is capable of being resolved into separate processes of verification, is that which renders it possible to form a great body of scientific truth, by adding together a vast number of truths, of which many men, at various times and by multiplied efforts, have satisfied themselves. The treasury of Science is constantly rich and abundant, because it accumulates the wealth which is thus gathered by so many, and reckoned over by so many more: and the dignity of Knowledge is no more lowered by the multiplicity of the tasks on which her servants are employed, and the narrow field of labour to which some confine themselves, than the rich merchant is degraded by the number of offices which it is necessary for him to maintain, and the minute articles of which he requires an exact statement from his accountants.

10. The analysis of doctrines inductively obtained, into their constituent facts, and the arrangement of them in such a form that the conclusiveness of the induction may be distinctly seen, may be termed *the Logic of Induction*. By *Logic* has generally been meant a system which teaches us so to arrange our reasonings that their truth or falsehood shall be evident in their form. In *deductive* reasonings, in which the general principles are assumed, and the question is concerning their application and combination in particular cases, the device which thus enables us to judge whether our reasonings are conclusive, is the *Syllogism*; and this *form*, along with the rules which belong to it, does in fact supply us with a criterion of deductive or demonstrative reasoning.

The *Inductive Table*, such as it is presented in the present chapter, in like manner supplies the means of ascertaining the truth of our *inductive* inferences, so far as the *form* in which our reasoning may be stated can afford such a criterion. Of course some care is requisite in order to reduce a train of demonstration into the form of a series of syllogisms; and certainly not less thought and attention are required for resolving all the main doctrines of any great department of science into a graduated table of co-ordinate and subordinate inductions. But in each case, when this task is once executed, the evidence or want of evidence of our conclusions appears immediately in a most luminous manner. In each step of induction, our Table enumerates the particular facts, and states the general theoretical truth which includes these and which these constitute. The special act of attention by which we satisfy ourselves that the facts *are* so included,—that the general truth *is* so constituted,—then affords little room for error, with moderate attention and clearness of thought.

11. We may find an example of this *act of attention* thus required, at any one of the steps of induction in our Tables; for instance, at the step in the early progress of astronomy at which it was inferred, that the earth is a globe, and that the sphere of the heavens performs a diurnal revolution round this globe of the earth. How was this established in the belief of the Greeks, and how is it fixed in our conviction? As to the globular form, we find that as we travel to the north, the apparent pole of the heavenly motions, and the constellations which are near it, seem to mount higher, and as we proceed southwards they descend. Again, if we proceed from two different points considerably to the east and west of each other, and travel directly northwards from each, as from the south of Spain to the north of Scotland, and

from Greece to Scandinavia, these two north and south lines will be much nearer to each other in their northern than in their southern parts. These and similar facts, as soon as they are clearly estimated and connected in the mind, are *seen to be consistent* with a convex surface of the earth, and with no other: and this notion is further confirmed by observing that the boundary of the earth's shadow upon the moon is always circular; it being supposed to be already established that the moon receives her light from the sun, and that lunar eclipses are caused by the interposition of the earth. As for the assertion of the diurnal revolution of the starry sphere, it is merely putting the visible phenomena in an exact geometrical form: and thus we establish and verify the doctrine of the revolution of the sphere of the heavens about the globe of the earth, by contemplating it so as to *see* that it does really and exactly include the particular facts from which it is collected.

We may, in like manner, illustrate this mode of verification by any of the other steps of the same Table. Thus if we take the great Induction of Copernicus, the heliocentric scheme of the solar system, we find it in the Table exhibited as including and explaining, *first*, the diurnal revolution just spoken of; *second*, the motions of the moon among the fixed stars; *third*, the motions of the planets with reference to the fixed stars and the sun; *fourth*, the motion of the sun in the ecliptic. And the scheme being clearly conceived, we *see* that all the particular facts *are* faithfully represented by it; and this agreement, along with the simplicity of the scheme, in which respect it is so far superior to any other conception of the solar system, persuade us that it is really the plan of nature.

In exactly the same way, if we attend to any of the several remarkable discoveries of Newton, which form

the principal steps in the latter part of the Table, as for instance, the proposition that the sun attracts all the planets with a force which varies inversely as the square of the distance, we find it proved by its including three other propositions previously established;—*first*, that the sun's mean force on different planets follows the specified variation (which is proved from Kepler's third law); *second*, that the force by which each planet is acted upon in different parts of its orbit tends to the sun (which is proved by the equable description of areas); *third*, that this force in different parts of the same orbit is also inversely as the square of the distance (which is proved from the elliptical form of the orbit). And the Newtonian generalization, when its consequences are mathematically traced, is *seen* to agree with each of these particular propositions, and thus is fully established.

12. But when we say that the more general proposition *includes* the several more particular ones, we must recollect what has before been said, that these particulars form the general truth, not by being merely enumerated and added together, but by being seen *in a new light*. No mere verbal recitation of the particulars can decide whether the general proposition is true; a special act of thought is requisite in order to determine how truly each is included in the supposed induction. In this respect the Inductive Table is not like a mere schedule of accounts, where the rightness of each part of the reckoning is tested by mere addition of the particulars. On the contrary, the Inductive truth is never the mere *sum* of the facts. It is made into something more by the introduction of a new mental element; and the mind, in order to be able to supply this element, must have peculiar endowments and discipline. Thus looking back at the instances noticed in the last article, how are we to see that a convex surface of the earth is

necessarily implied by the convergence of meridians towards the north, or by the visible descent of the north pole of the heavens as we travel south? Manifestly, the student, in order to see this, must have clear conceptions of the relations of space, either naturally inherent in his mind, or established there by geometrical cultivation,—by studying the properties of circles and spheres. When he is so prepared, he will feel the force of the expressions we have used, that the facts just mentioned are *seen to be consistent* with a globular form of the earth; but without such aptitude he will not see this consistency: and if this be so, the mere assertion of it in words will not avail him in satisfying himself of the truth of the proposition.

In like manner, in order to perceive the force of the Copernican induction, the student must have his mind so disciplined by geometrical studies, or otherwise, that he sees clearly how absolute motion and relative motion would alike produce apparent motion. He must have learnt to cast away all prejudices arising from the seeming fixity of the earth; and then he will see that there is nothing which stands in the way of the induction, while there is much which is on its side. And in the same manner the Newtonian induction of the law of the sun's force from the elliptical form of the orbit, will be evidently satisfactory to him only who has such an insight into Mechanics as to see that a curvilinear path must arise from a constantly deflecting force; and who is able to follow the steps of geometrical reasoning by which, from the properties of the ellipse, Newton proves this deflection to be in the proportion in which he asserts the force to be. And thus in all cases the inductive truth must indeed be verified by comparing it with the particular facts; but then this comparison is possible for him only whose mind is properly

disciplined and prepared in the use of those conceptions, which, in addition to the facts, the act of induction requires.

13. In the Tables some indication is given, at several of the steps, of the act which the mind must thus perform, besides the mere conjunction of facts, in order to attain to the inductive truth. Thus in the cases of the Newtonian inductions just spoken of, the inferences are stated to be made "By Mechanics;" and in the case of the Copernican induction, it is said that, "By the nature of motion, the apparent motion is the same, whether the heavens or the earth have a diurnal motion; and the latter is more simple." But these verbal statements are to be understood as mere hints\*: they cannot supersede the necessity of the student's contemplating for himself the mechanical principles and the nature of motion thus referred to.

14. In the Common or Syllogistic Logic, a certain *Formula* of language is used in stating the reasoning, and is useful in enabling us more readily to apply the Criterion of Form to alleged demonstrations. This formula is the usual Syllogism; with its members, Major Premiss, Minor Premiss, and Conclusion. It may naturally be asked whether in Inductive Logic there is any such Formula? whether there is any standard form of words in which we may most properly express the inference of a general truth from particular facts?

At first it might be supposed that the formula of Inductive Logic need only be of this kind: "These particulars, and all known particulars of the same kind, are exactly included in the following general proposition." But a moment's reflection on what has just been said will show us that this is not sufficient: for the particulars are not merely *included* in the general proposition. It

\* In the Inductive Tables they are marked by an asterisk



is not enough that they appertain to it by enumeration. It is, for instance, no adequate example of Induction to say, "Mercury describes an elliptical path, so does Venus, so do the Earth, Mars, Jupiter, Saturn, Uranus; therefore all the Planets describe elliptical paths." This is, as we have seen, the mode of stating the *evidence* when the proposition is once suggested; but the Inductive step consists in the *suggestion* of a conception not before apparent. When Kepler, after trying to connect the observed places of the planet Mars in many other ways, found at last that the conception of an ellipse would include them all, he obtained a truth by induction: for this conclusion was not obviously included in the phenomena, and had not been applied to these facts previously. Thus in our Formula, besides stating that the particulars are included in the general proposition, we must also imply that the generality is constituted by a new Conception,—new at least in its application.

Hence our Inductive Formula might be something like the following: "These particulars, and all known particulars of the same kind, are exactly expressed by adopting the Conceptions and Statement of the following Proposition." It is of course requisite that the Conceptions should be perfectly clear, and should precisely embrace the facts, according to the explanation we have already given of those conditions.

15. It may happen, as we have already stated, that the Explication of a Conception, by which it acquires its due distinctness, leads to a Definition, which Definition may be taken as the summary and total result of the intellectual efforts to which this distinctness is due. In such cases, the Formula of Induction may be modified according to this condition; and we may state the inference by saying, after an enumeration and analysis of the appropriate facts, "These facts are completely and dis-

tinctly expressed by adopting the following Definition and Proposition."

This Formula has been adopted in stating the Inductive Propositions which constitute the basis of the science of Mechanics, in a work intitled *The Mechanical Euclid*. The fundamental truths of the subject are expressed in *Inductive Pairs* of Assertions, consisting each of a Definition and a Proposition, such as the following:

DEF.—A *Uniform Force* is that which acting in the direction of the body's motion, adds or subtracts equal velocities in equal times.

PROP.—Gravity is a Uniform Force.

Again,

DEF.—Two *Motions* are *compounded* when each produces its separate effect in a direction parallel to itself.

PROP.—When any Force acts upon a body in motion, the motion which the Force would produce in the body at rest is compounded with the previous motion of the body.

And in like manner in other cases.

In these cases the proposition is, of course, established, and the definition realized, by an enumeration of the facts. And in the case of inferences made in such a form, the Definition of the Conception and the Assertion of the Truth are both requisite and are correlative to one another. Each of the two steps contains the verification and justification of the other. The Proposition derives its meaning from the Definition; the Definition derives its reality from the Proposition. If they are separated, the Definition is arbitrary or empty, the Proposition vague or ambiguous.

16. But it must be observed that neither of the preceding Formulæ expresses the full cogency of the inductive proof. They declare only that the results *can* be

clearly explained and rigorously deduced by the employment of a certain Definition and a certain Proposition. But in order to make the conclusion demonstrative, which in perfect examples of Induction it is, we ought to be able to declare that the results can be clearly explained and rigorously declared *only* by the Definition and Proposition which we adopt. And in reality, the conviction of the sound inductive reasoner does reach to this point. The Mathematician asserts the Laws of Motion, seeing clearly that they (or laws equivalent to them) afford the only means of clearly expressing and deducing the actual facts. But this conviction, that the inductive inference is not only consistent with the facts, but necessary, finds its place in the mind gradually, as the contemplation of the consequences of the proposition, and the various relations of the facts, becomes steady and familiar. It is scarcely possible for the student at once to satisfy himself that the inference is thus inevitable. And when he arrives at this conviction, he sees also, in many cases at least, that there may be other ways of expressing the substance of the truth established, besides that special Proposition which he has under his notice.

We may, therefore, without impropriety, renounce the undertaking of conveying in our formula this final conviction of the necessary truth of our inference. We may leave it to be thought, without insisting upon saying it, that in such cases what *can* be true, *is* true. But if we wish to express the ultimate significance of the Inductive Act of thought, we may take as our Formula for the Colligation of Facts by Induction, this:—"The several Facts are exactly expressed as one Fact if, *and only if*, we adopt the Conception and the Assertion" of the inductive inference.

17. I have said that the mind must be properly dis-

ciplined in order that it may see the necessary connexion between the facts and the general proposition in which they are included. And the perception of this connexion, though treated as *one step* in our inductive inference, may imply *many steps* of demonstrative proof. The connexion is this, that the particular case is included in the general one, that is, may be *deduced* from it: but this deduction may often require many links of reasoning. Thus in the case of the inference of the law of the force from the elliptical form of the orbit by Newton, the proof that in the ellipse the deflection from the tangent is inversely as the square of the distance from the focus of the ellipse, is a ratiocination consisting of several steps, and involving several properties of Conic Sections; these properties being supposed to be previously established by a geometrical system of demonstration on the special subject of the Conic Sections. In this and similar cases the Induction involves many steps of Deduction. And in such cases, although the Inductive Step, the Invention of the Conception, is really the most important, yet since, when once made, it occupies a familiar place in men's minds; and since the Deductive Demonstration is of considerable length and requires intellectual effort to follow it at every step; men often admire the deductive part of the proposition, the geometrical or algebraical demonstration, far more than that part in which the philosophical merit really resides.

18. Deductive reasoning is virtually a collection of syllogisms, as has already been stated; and in such reasoning, the general principles, the Definitions and Axioms, necessarily stand at the *beginning* of the demonstration. In an inductive inference, the Definitions and Principles are the *final result* of the reasoning, the ultimate effect of the proof. Hence when an Inductive Proposition is to be established by a proof involving several steps of

demonstrative reasoning, the enunciation of the Proposition will contain, explicitly or implicitly, principles which the demonstration proceeds upon as axioms, but which are really inductive inferences. Thus in order to prove that the force which retains a planet in an ellipse varies inversely as the square of the distance, it is taken for granted that the Laws of Motion are true, and that they apply to the planets. Yet the doctrine that this is so, as well as the law of the force, were established only by this and the like demonstrations. The doctrine which is the *hypothesis* of the deductive reasoning, is the *inference* of the inductive process. The special facts which are the basis of the inductive inference, are the conclusion of the train of deduction. And in this manner the deduction establishes the induction. The principle which we gather from the facts is true, because the facts can be derived from it by rigorous demonstration. Induction moves upwards, and deduction downwards, on the same stair.

But still there is a great difference in the character of their movements. Deduction descends steadily and methodically, step by step: Induction mounts by a leap which is out of the reach of method. She bounds to the top of the stair at once; and then it is the business of Deduction, by trying each step in order, to establish the solidity of her companion's footing. Yet these must be processes of the same mind. The Inductive Intellect makes an assertion which is subsequently justified by demonstration; and it shows its sagacity, its peculiar character, by enunciating the proposition when as yet the demonstration does not exist: but then it shows that it *is* sagacity, by also producing the demonstration.

It has been said that inductive and deductive reasoning are contrary in their scheme; that in Deduction we infer particular from general truths; while in Induction

we infer general from particular: that Deduction consists of many steps, in each of which we apply known general propositions in particular cases; while in Induction we have a single step, in which we pass from many particular truths to one general proposition. And this is truly said; but though contrary in their motions, the two are the operation of the same mind travelling over the same ground. Deduction is a necessary part of Induction. Deduction justifies by calculation what Induction had happily guessed. Induction recognizes the ore of truth by its weight; Deduction confirms the recognition by chemical analysis. Every step of Induction must be confirmed by rigorous deductive reasoning, followed into such detail as the nature and complexity of the relations (whether of quantity or any other) render requisite. If not so justified by the supposed discoverer, it is *not* Induction.

19. Such Tabular arrangements of propositions as we have constructed may be considered as the *Criterion of Truth* for the doctrines which they include. They are the Criterion of Inductive Truth, in the same sense in which Syllogistic Demonstration is the Criterion of Necessary Truth,—of the certainty of conclusions, depending upon evident First Principles. And that such Tables are really a Criterion of the truth of the propositions which they contain, will be plain by examining their structure. For if the connexion which the inductive process assumes be ascertained to be in each case real and true, the assertion of the general proposition merely collects together ascertained truths; and in like manner each of those more particular propositions is true, because it merely expresses collectively more special facts: so that the most general theory is only the assertion of a great body of facts, duly classified and

subordinated. When we assert the truth of the Copernican theory of the motions of the solar system, or of the Newtonian theory of the forces by which they are caused, we merely assert the groups of propositions which, in the Table of Astronomical Induction, are included in these doctrines; and ultimately, we may consider ourselves as merely asserting at once so many Facts, and therefore, of course, expressing an indisputable truth.

20. At any one of these steps of Induction in the Table, the inductive proposition is a *Theory* with regard to the Facts which it includes, while it is to be looked upon as a *Fact* with respect to the higher generalizations in which it is included. In any other sense, as was formerly shown, the opposition of Fact and Theory is untenable, and leads to endless perplexity and debate. Is it a Fact or a Theory that the planet Mars revolves in an Ellipse about the Sun? To Kepler, employed in endeavouring to combine the separate observations by the Conception of an Ellipse, it is a Theory; to Newton, engaged in inferring the law of force from a knowledge of the elliptical motion, it is a Fact. There are, as we have already seen, no special attributes of Theory and Fact which distinguish them from one another. Facts are phenomena apprehended by the aid of conceptions and mental acts, as Theories also are. We commonly call our observations *Facts*, when we apply, without effort or consciousness, conceptions perfectly familiar to us: while we speak of Theories, when we have previously contemplated the Facts and the connecting Conception separately, and have made the connexion by a conscious mental act. The real difference is a difference of relation; as the same proposition in a demonstration is the *premiss* of one syllogism and the *conclusion* in another;

—as the same person is a father and a son. Propositions are Facts and Theories, according as they stand above or below the Inductive Brackets of our Tables.

21. To obviate mistakes I may remark that the terms *higher* and *lower*, when used of generalizations, are unavoidably represented by their opposites in our Inductive Tables. The highest generalization is that which includes all others; and this stands the lowest on our page, because, reading downwards, that is the place which we last reach.

There is a distinction of the knowledge acquired by Scientific Induction into two kinds, which is so important that we shall consider it in the succeeding chapter.

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## CHAPTER VII.

### OF LAWS OF PHENOMENA AND OF CAUSES.

1. IN the first attempts at acquiring an exact and connected knowledge of the appearances and operations which nature presents, men went no further than to learn *what* takes place, not *why* it occurs. They discovered an Order which the phenomena follow, Rules which they obey; but they did not come in sight of the Powers by which these rules are determined, the Causes of which this order is the effect. Thus, for example, they found that many of the celestial motions took place as if the sun and stars were carried round by the revolutions of certain celestial spheres; but what causes kept these spheres in constant motion, they were never able to explain. In like manner in modern times, Kepler discovered that the planets describe ellipses, before Newton explained why they select this particular curve, and describe it in a particular manner. The laws of reflec-



tion, refraction, dispersion, and other properties of light have long been known; the causes of these laws are at present under discussion. And the same might be said of many other sciences. The discovery of *the Laws of Phenomena* is, in all cases, the first step in exact knowledge; these Laws may often for a long period constitute the whole of our science; and it is always a matter requiring great talents and great efforts, to advance to a knowledge of the *Causes* of the phenomena.

Hence the larger part of our knowledge of nature, at least of the certain portion of it, consists of the knowledge of the Laws of Phenomena. In Astronomy indeed, besides knowing the rules which guide the appearances, and resolving them into the real motions from which they arise, we can refer these motions to the forces which produce them. In Optics, we have become acquainted with a vast number of laws by which varied and beautiful phenomena are governed; and perhaps we may assume, since the evidence of the undulatory theory has been so fully developed, that we know also the Causes of the Phenomena. But in a large class of sciences, while we have learnt many Laws of Phenomena, the causes by which these are produced are still unknown or disputed. Are we to ascribe to the operation of a fluid or fluids, and if so, in what manner, the facts of heat, magnetism, electricity, galvanism? What are the forces by which the elements of chemical compounds are held together? What are the forces, of a higher order, as we cannot help believing, by which the course of vital action in organized bodies is kept up? In these and other cases, we have extensive departments of science; but we are as yet unable to trace the effects to their causes; and our science, so far as it is positive and certain, consists entirely of the laws of phenomena.

2. In those cases in which we have a division of the

science which teaches us the doctrine of the causes, as well as one which states the rules which the effects follow, I have distinguished the two portions of the science by certain terms. I have thus spoken of *Formal Astronomy* and *Physical Astronomy*. The latter phrase has long been commonly employed to describe that department of Astronomy which deals with those forces by which the heavenly bodies are guided in their motions; the former adjective appears well suited to describe a collection of rules depending on those ideas of space, time, position, number, which are, as we have already said, the *forms* of our apprehension of phenomena. The laws of phenomena may be considered as *formulæ*, expressing results in terms of those ideas. In like manner, I have spoken of *Formal Optics* and *Physical Optics*; the latter division including all speculations concerning the machinery by which the effects are produced. *Formal Acoustics* and *Physical Acoustics* may be distinguished in like manner, although these two portions of science have been a good deal mixed together by most of those who have treated of them. *Formal Thermotics*, the knowledge of the laws of the phenomena of heat, ought in like manner to lead to *Physical Thermotics*, or the *Theory of Heat* with reference to the mode in which its effects are produced;—a branch of science which as yet can hardly be said to exist.

3. What *kinds of cause* are we to admit in science? This is an important, and by no means an easy question. In order to answer it, we must consider in what manner our progress in the knowledge of causes has hitherto been made. By far the most conspicuous instance of success in such researches, is the discovery of the causes of the motions of the heavenly bodies. In this case, after the formal laws of the motions,—their conditions as to space and time,—had become known, men were

enabled to go a step further; to reduce them to the familiar and general cause of motion—mechanical force; and to determine the laws which this force follows. That this was a step in addition to the knowledge previously possessed, and that it was a real and peculiar truth, will not be contested. And a step in any other subject which should be analogous to this in astronomy;—a discovery of causes and forces as certain and clear as the discovery of universal gravitation;—would undoubtedly be a vast advance upon a body of science consisting only of the laws of phenomena.

4. But although physical astronomy may well be taken as a standard in estimating the value and magnitude of the advance from the knowledge of phenomena to the knowledge of causes; the peculiar features of the transition from formal to physical science in that subject must not be allowed to limit too narrowly our views of the nature of this transition in other cases. We are not, for example, to consider that the step which leads us to the knowledge of causes in any province of nature must necessarily consist in the discovery of centers of forces, and collections of such centers, by which the effects are produced. The discovery of the causes of phenomena may imply the detection of a fluid by whose undulations, or other operations, the results are occasioned. The phenomena of acoustics are, we know, produced in this manner by the air; and in the cases of light, heat, magnetism, and others, even if we reject all the theories of such fluids which have hitherto been proposed, we still cannot deny that such theories are intelligible and possible, as the discussions concerning them have shown. Nor can it be doubted that if the assumption of such a fluid, in any case, were as well evidenced as the doctrine of universal gravitation is, it must be considered as a highly valuable theory.

5. But again; not only must we, in aiming at the formation of a Causal Section in each Science of Phenomena, consider fluids and their various modes of operation admissible, as well as centers of mechanical force; but we must be prepared, if it be necessary, to consider the forces, or powers to which we refer the phenomena, under still more general aspects, and invested with characters different from mere mechanical force. For example; the forces by which the chemical elements of bodies are bound together, and from which arise, both their sensible texture, their crystalline form, and their chemical composition, are certainly forces of a very different nature from the mere attraction of matter according to its mass. The powers of assimilation and reproduction in plants and animals are obviously still more removed from mere mechanism; yet these powers are not on that account less real, nor a less fit and worthy subject of scientific inquiry.

6. In fact, these forces—mechanical, chemical and vital,—as we advance from one to the other, each bring into our consideration new characters; and what these characters are, has appeared in the survey which we have made of the Fundamental Ideas of the various sciences. It was then shown that the forces by which chemical effects are produced necessarily involve the Idea of Polarity,—they are polar forces; the particles tend together in virtue of opposite properties which in the combination neutralize each other. Hence, in attempting to advance to a theory of Causes in chemistry, our task is by no means to invent laws of *mechanical* force, and collections of forces, by which the effects may be produced. We know beforehand that no such attempt can succeed. Our aim must be to conceive such new kinds of force, including polarity among their characters, as may best render the results intelligible.

7. Thus in advancing to a Science of Cause in any subject, the labour and the struggle is, not to analyze the phenomena according to any preconceived and already familiar ideas, but to form distinctly new conceptions, such as do really carry us to a more intimate view of the processes of nature. Thus in the case of astronomy, the obstacle which deferred the discovery of the true causes from the time of Kepler to that of Newton, was the difficulty of taking hold of mechanical conceptions and axioms with sufficient clearness and steadiness; which, during the whole of that interval, mathematicians were learning to do. In the question of causation which now lies most immediately in the path of science, that of the causes of electrical and chemical phenomena, the business of rightly fixing and limiting the conception of polarity, is the proper object of the efforts of discoverers. Accordingly a large portion of Mr. Faraday's recent labours\* is directed, not to the attempt at discovering new laws of phenomena, but to the task of throwing light upon the conception of polarity, and of showing how it must be understood, so that it shall include electrical induction and other phenomena, which have commonly been ascribed to forces acting mechanically at a distance. He is by no means content, nor would it answer the ends of science that he should be, with stating the results of his experiments; he is constantly, in every page, pointing out the interpretation of his experiments, and showing how the conception of polar forces enters into this interpretation. "I shall," he says†, "use every opportunity which presents itself of returning to that strong test of truth, experiment; but," he adds, "I shall necessarily have occasion to speak

\* Eleventh, Twelfth, and Thirteenth Series of Researches, *Phil. Trans.* 1837 and 8.

† Art. 1318.

theoretically, and even hypothetically." His hypothesis that electrical inductive action always takes place by means of a continuous line of polarized particles, and not by attraction and repulsion at a distance, if established, cannot fail to be a great step on our way towards a knowledge of causes, as well as phenomena, in the subjects under his consideration.

8. The process of obtaining new conceptions is, to most minds, far more unwelcome than any labour in employing old ideas. The effort is indeed painful and oppressive; it is feeling in the dark for an object which we cannot find. Hence it is not surprising that we should far more willingly proceed to seek for new causes by applying conceptions borrowed from old ones. Men were familiar with solid frames, and with whirlpools of fluid, when they had not learnt to form any clear conception of attraction at a distance. Hence they at first imagined the heavenly motions to be caused by crystalline spheres, and vortices. At length they were taught to conceive central forces, and then they reduced the solar system to these. But having done this, they fancied that all the rest of the machinery of nature must be central forces. We find Newton expressing this conviction\*, and the mathematicians of the last century acted upon it very extensively. We may especially remark Laplace's labours in this field. Having explained, by such forces, the phenomena of capillary attraction, he attempted to apply the same kind of explanation to the reflection, refraction, and double refraction of light;—to the constitution of gases;—the operation of heat. It was soon seen that the explanation of refraction was arbitrary, and that of double refraction illusory; while polarization entirely eluded the grasp of this machinery. Centers of force would no longer represent the modes of causation

\* *Multa me movent, &c.*, Pref. to the *Principia*, already quoted.

which belonged to the phenomena. Polarization required some other contrivance, such as the undulatory theory supplied. No theory of light can be of any avail in which the fundamental idea of polarity is not clearly exhibited.

9. The sciences of magnetism and electricity have given rise to theories in which this relation of polarity is exhibited by means of two opposite fluids†;—a positive and a negative fluid, or a vitreous and a resinous, for electricity, and a boreal and an austral fluid for magnetism. The hypothesis of such fluids gives results agreeing in a remarkable manner with the facts and their measures, as Coulomb and others have shown. It may be asked how far we may, in such a case, suppose that we have discovered the true cause of the phenomena, and whether it is sufficiently proved that these fluids really exist. The right answer seems to be, that the hypothesis certainly represents the truth so far as regards the polar relation of the two energies, and the laws of the attractive and repulsive forces of the particles in which these energies reside; but that we are not entitled to assume that the vehicles of these energies possess other attributes of material fluids, or that the forces thus ascribed to the particles are the primary elementary forces from which the action originates. We are the more bound to place this cautious limit to our acceptance of the Coulombian theory, since in electricity Faraday has in vain endeavoured to bring into view one of the polar fluids without the other: whereas such a result ought to be possible if there were two separable fluids. The impossibility of this separate exhibition of one fluid appears to show that the fluids are real only so far as they are polar. And Faraday's view above mentioned, according to which the attractions at a distance are resolved into the action of lines of polarized

\* *Hist. Ind. Sci.*, B. XI. c. ii.

particles of air, appears still further to show that the conceptions hitherto entertained of electrical forces, according to the Coulombian theory, do not penetrate to the real and intimate nature of the causation belonging to this case.

10. Since it is thus difficult to know when we have seized the true cause of the phenomena in any department of science, it may appear to some persons that physical inquirers are imprudent and unphilosophical in undertaking this research of causes; and that it would be safer and wiser to confine ourselves to the investigation of the laws of phenomena, in which field the knowledge which we obtain is definite and certain. Hence there have not been wanting those who have laid it down as a maxim that "science must study only the laws of phenomena, and never the mode of production\*." But it is easy to see that such a maxim would confine the breadth and depth of scientific inquiries to a most scanty and miserable limit. Indeed, such a rule would defeat its own object; for the laws of phenomena, in many cases, cannot be even expressed or understood without some hypothesis respecting their mode of production. How could the phenomena of polarization have been conceived or reasoned upon, except by imagining a polar arrangement of particles, or transverse vibrations, or some equivalent hypothesis? The doctrines of fits of easy transmission, the doctrine of moveable polarization, and the like, even when erroneous as representing the whole of the phenomena, were still useful in combining some of them into laws; and without some such hypotheses the facts could not have been followed out. The doctrine of a fluid caloric may be false; but without imagining such a fluid, how could the movement of heat from one part of a body to another be conceived? It may

\* Comte, *Philosophie Positive*.



be replied that Fourier, Laplace, Poisson, who have principally cultivated the Theory of Heat, have not conceived it as a fluid, but have referred conduction to the radiation of the molecules of bodies, which they suppose to be separate points. But this molecular constitution of bodies is itself an assumption of the mode in which the phenomena are produced; and the radiation of heat suggests inquiries concerning a fluid emanation, no less than its conduction does. In like manner, the attempts to connect the laws of phenomena of heat and of gases, have led to hypotheses respecting the constitution of gases, and the combination of their particles with those of caloric, which hypotheses may be false, but are probably the best means of discovering the truth.

To debar science from inquiries like these, on the ground that it is her business to inquire into facts, and not to speculate about causes, is a curious example of that barren caution which hopes for truth without daring to venture upon the quest of it. This temper would have stopped with Kepler's discoveries, and would have refused to go on with Newton to inquire into the mode in which the phenomena are produced. It would have stopped with Newton's optical facts, and would have refused to go on with him and his successors to inquire into the mode in which these phenomena are produced. And, as we have abundantly shown, it would, on that very account, have failed in seeing what the phenomena really are.

In many subjects the attempt to study the laws of phenomena, independently of any speculations respecting the causes which have produced them, is neither possible for human intelligence nor for human temper. Men cannot contemplate the phenomena without clothing them in terms of some hypothesis, and will not be schooled to

suppress the questionings which at every moment rise up within them concerning the causes of the phenomena. Who can attend to the appearances which come under the notice of the geologist;—strata regularly bedded, full of the remains of animals such as now live in the depths of the ocean, raised to the tops of mountains, broken, contorted, mixed with rocks such as still flow from the mouths of volcanos;—who can see phenomena like these, and imagine that he best promotes the progress of our knowledge of the earth's history, by noting down the facts, and abstaining from all inquiry whether these are really proofs of past states of the earth and of subterraneous forces, or merely an accidental imitation of the effects of such causes? In this and similar cases, to proscribe the inquiry into causes would be to annihilate the science.

Finally, this caution does not even gain its own single end, the escape from hypotheses. For, as we have said, those who will not seek for new and appropriate causes of newly-studied phenomena, are almost inevitably led to ascribe the facts to modifications of causes already familiar. They may declare that they will not hear of such causes as vital powers, elective affinities, electric, or calorific, or luminiferous ethers or fluids; but they will not the less on that account assume hypotheses equally unauthorized; for instance—universal mechanical forces; a molecular constitution of bodies; solid, hard, inert matter;—and will apply these hypotheses in a manner which is arbitrary in itself as well as quite insufficient for its purpose.

11. It appears, then, to be required, both by the analogy of the most successful efforts of science in past times and by the irrepressible speculative powers of the human mind, that we should attempt to discover both the *laws of phenomena*, and their *causes*. In every de-

partment of science, when prosecuted far enough, these two great steps of investigation must succeed each other. The laws of phenomena must be known before we can speculate concerning causes; the causes must be inquired into when the phenomena have been reduced to rule. In both these speculations the suppositions and conceptions which occur must be constantly tested by reference to observation and experiment. In both we must, as far as possible, devise hypotheses which, when we thus test them, display those characters of truth of which we have already spoken;—an agreement with facts such as will stand the most patient and rigid inquiry; a provision for predicting truly the results of untried cases; a consistence of inductions from various classes of facts; and a progressive tendency of the scheme to simplicity and unity.

We shall attempt hereafter to give several rules of a more precise and detailed kind for the discovery of the causes, and still more, of the laws of phenomena. But it will be useful in the first place to point out the Classification of the Sciences which results from the principles already established in this word. And for this purpose we must previously decide the question, whether the practical Arts, as Medicine and Engineering, must be included in our list of Sciences.

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## CHAPTER VIII.

### OF ART AND SCIENCE.

1. THE distinction of Arts and Sciences very materially affects all classifications of the departments of Human Knowledge. It is often maintained, expressly or tacitly, that the Arts are a part of our knowledge, in the

same sense in which the Sciences are so ; and that Art is the application of Science to the purposes of practical life. It will be found that these views require some correction, when we understand *Science* in the exact sense in which we have throughout endeavoured to contemplate it, and in which alone our examination of its nature can instruct us in the true foundations of our knowledge.

When we cast our eyes upon the early stages of the histories of nations, we cannot fail to be struck with the consideration, that in many countries the Arts of life already appear, at least in some rude form or other, when, as yet, nothing of science exists. A practical knowledge of astronomy, such as enables them to reckon months and years, is found among all nations except the mere savages. A practical knowledge of mechanics must have existed in those nations which have left us the gigantic monuments of early architecture. The pyramids and temples of Egypt and Nubia, the Cyclopean walls of Italy and Greece, the temples of Magna Græcia and Sicily, the obelisks and edifices of India, the cromlechs and Druidical circles of countries formerly Celtic,—must have demanded no small practical mechanical skill and power. Yet those modes of reckoning time must have preceded the rise of speculative astronomy ; these structures must have been erected before the theory of mechanics was known. To suppose, as some have done, a great body of science, now lost, to have existed in the remote ages to which these remains belong, is not only quite gratuitous and contrary to all analogy, but is a supposition which cannot be extended so far as to explain all such cases. For it is impossible to imagine that *every* art has been preceded by the science which renders a reason for its processes. Certainly men formed wine from the grape, before they possessed a science of fermentation ;

the first instructor of every artificer in brass and iron can hardly be supposed to have taught the chemistry of metals as a science; the inventor of the square and the compasses had probably no more knowledge of demonstrated geometry than have the artisans who now use those implements; and finally, the use of speech, the employment of the inflections and combinations of words, must needs be assumed as having been prior to any general view of the nature and analogy of language. Even at this moment, the greater part of the arts which exist in the world are not accompanied by the sciences on which they theoretically depend. Who shall state to us the general chemical truths to which the manufactures of glass, and porcelain, and iron, and brass, owe their existence? Do not almost all artisans practise many successful artifices long before science explains the ground of the process? Do not arts at this day exist, in a high state of perfection, in countries in which there is no science, as China and India? These countries and many others have no theories of mechanics, of optics, of chemistry, of physiology; yet they construct and use mechanical and optical instruments, make chemical combinations, take advantage of physiological laws. It is too evident to need further illustration that art may exist without science;—that it has usually been anterior to it, and even now commonly advances independently, leaving science to follow as it can.

2. We here mean by *Science*, that exact, general, speculative knowledge, of which we have, throughout this work, been endeavouring to exhibit the nature and rules. Between such science and the *practical Arts* of life, the points of difference are sufficiently manifest. The object of Science is *Knowledge*; the object of Art are *Works*. The latter is satisfied with producing its

material results ; to the former, the operations of matter, whether natural or artificial, are interesting only so far as they can be embraced by intelligible principles. The end of art is the beginning of science ; for when it is seen *what* is done, then comes the question *why* it is done. Art may have fixed general rules, stated in words ; but she has these merely as means to an end : to Science, the propositions which she obtains are each, in itself, a sufficient end of the effort by which it is acquired. When Art has brought forth her product, her task is finished ; Science is constantly led by one step of her path to another. Each proposition which she obtains impels her to go onwards to other propositions more general, more profound, more simple. Art puts elements together, without caring to know what they are, or why they coalesce. Science analyzes the compound, and at every such step strives not only to perform, but to understand the analysis. Art advances in proportion as she becomes able to bring forth products more multiplied, more complex, more various ; but Science, straining her eyes to penetrate more and more deeply into the nature of things, reckons her success in proportion as she sees, in all the phenomena, however multiplied, complex, and varied, the results of one or two simple and general laws.

3. There are many acts which man, as well as animals, performs by the guidance of nature, without seeing or seeking the reason why he does so ; as the acts by which he balances himself in standing or moving, and those by which he judges of the form and position of the objects around him. These actions have their reason in the principles of geometry and mechanics ; but of such reasons he who thus acts is unaware : he works blindly, under the impulse of an unknown principle which we call *Instinct*. When man's speculative nature seeks and finds

the reasons why he should act thus or thus;—why he should stretch out his arm to prevent his falling, or assign a certain position to an object in consequence of the angles under which it is seen;—he may perform the same actions as before, but they are then done by the aid of a different faculty, which, for the sake of distinction, we may call *Insight*. Instinct is a purely active principle; it is seen in deeds alone; it has no power of looking inwards; it asks no questions; it has no tendency to discover reasons or rules; it is the opposite of Insight.

4. Art is not identical with Instinct: on the contrary, there are broad differences. Instinct is stationary; Art is progressive. Instinct is mute; it acts, but gives no rules for acting: Art can speak; she can lay down rules. But though Art is thus separate from Instinct, she is not essentially combined with Insight. She can see what to do, but she needs not to see why it is done. She may lay down rules, but it is not her business to give reasons. When man makes *that* his employment, he enters upon the domain of science. Art takes the phenomena and laws of nature as she finds them: that they are multiplied, complex, capricious, incoherent, disturbs her not. She is content that the rules of nature's operations should be perfectly arbitrary and unintelligible, provided they are constant, so that she can depend upon their effects. But Science is impatient of all appearance of caprice, inconsistency, irregularity, in nature. She will not believe in the existence of such characters. She resolves one apparent anomaly after another; her task is not ended till every thing is so plain and simple, that she is tempted to believe she sees that it could by no possibility have been otherwise than it is.

5. It may be said that, after all, Art does really involve the knowledge which Science delivers;—that the

artisan who raises large weights, practically *knows* the properties of the mechanical powers;—that he who manufactures chemical compounds is virtually acquainted with the laws of chemical combination. To this we reply, that it might on the same grounds be asserted, that he who acts upon the principle that two sides of a triangle are greater than the third is really acquainted with geometry; and that he who balances himself on one foot knows the properties of the center of gravity. But this is an acquaintance with geometry and mechanics which even brute animals possess. It is evident that it is not of such knowledge as this that we have here to treat. It is plain that this mode of possessing principles is altogether different from that contemplation of them on which science is founded. We neglect the most essential and manifest differences, if we confound our unconscious assumptions with our demonstrative reasonings.

6. The real state of the case is, that the principles which Art *involves*, Science alone *evolves*. The truths on which the success of Art depends, lurk in the artist's mind in an undeveloped state; guiding his hand, stimulating his invention, balancing his judgment, but not appearing in the form of enunciated propositions. Principles are not to him direct objects of meditation: they are secret Powers of Nature, to which the forms which tenant the world owe their constancy, their movements, their changes, their luxuriant and varied growth, but which he can nowhere directly contemplate. That the creative and directive principles which have their lodgment in the artist's mind, when *unfolded* by our speculative powers into systematic shape, become science, is true; but it is precisely this process of development which gives to them their character of science. In practical Art, principles are unseen guides, leading us by invisible strings through paths where the end alone is



looked at: it is for Science to direct and purge our vision so that these airy ties, these principles and laws, generalizations and theories, become distinct objects of vision. Many may feel the intellectual monitor, but it is only to her favourite heroes that the Goddess of Wisdom visibly reveals herself.

7. Thus Art, in its earlier stages at least, is widely different from Science, independent of it, and anterior to it. At a later period, no doubt, Art may borrow aid from Science; and the discoveries of the philosopher may be of great value to the manufacturer and the artist. But even then, this application forms no essential part of the science: the interest which belongs to it is not an intellectual interest. The augmentation of human power and convenience may impel or reward the physical philosopher; but the processes by which man's repasts are rendered more delicious, his journeys more rapid, his weapons more terrible, are not, therefore, Science. They may involve principles which are of the highest interest to science; but as the advantage is not practically more precious because it results from a beautiful theory, so the theoretical principle has no more conspicuous place in science because it leads to convenient practical consequences. The nature of science is purely intellectual; knowledge alone,—exact general truth,—is her object; and we cannot mix with such materials, as matters of the same kind, the merely empirical maxims of art, without introducing endless confusion into the subject, and making it impossible to attain any solid footing in our philosophy.

8. I shall therefore not place, in our Classification of the Sciences, the Arts, as has generally been done; nor shall I notice the applications of sciences to art, as forming any separate portion of each science. The sciences, considered as bodies of general speculative

truths, are what we are here concerned with ; and applications of such truths, whether useful or useless, are important to us only as illustrations and examples. Whatever place in human knowledge the Practical Arts may hold, they are not Sciences. And it is only by this rigorous separation of the Practical from the Theoretical, that we can arrive at any solid conclusions respecting the nature of truth, and the mode of arriving at it, such as it is our object to attain.

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## CHAPTER IX.

### OF THE CLASSIFICATION OF SCIENCES.

1. THE Classification of Sciences has its chief use in pointing out to us the extent of our powers of arriving at truth, and the analogies which may obtain between those certain and lucid portions of knowledge with which we are here concerned, and those other portions, of a very different interest and evidence, which we here purposely abstain to touch upon. The classification of human knowledge will, therefore, have a more peculiar importance when we can include in it the moral, political, and metaphysical, as well as the physical portions of our knowledge. But such a survey does not belong to our present undertaking : and a general view of the connexion and order of the branches of sciences which our review has hitherto included, will even now possess some interest ; and may serve hereafter as an introduction to a more complete scheme of the general body of human knowledge.

2. In this, as in any other case, a sound classification must be the result, not of any assumed principles imperatively applied to the subject, but of an examination of

the objects to be classified;—of an analysis of them into the principles in which they agree and differ. The Classification of Sciences must result from the consideration of their nature and contents. Accordingly, that review of the sciences in which the History of them engaged us, led to a Classification, of which the main features are indicated in that work. The Classification thus obtained, depends neither upon the faculties of the mind to which the separate parts of our knowledge owe their origin, nor upon the objects which each science contemplates; but upon a more natural and fundamental element;—namely, the *Ideas* which each science involves. The Ideas regulate and connect the facts, and are the foundations of the reasoning, in each science: and having in the present work more fully examined these Ideas, we are now prepared to state here the classification to which they lead. If we have rightly traced each science to the Conceptions which are really fundamental *with regard to it*, and which give rise to the first principles on which it depends, it is not necessary for our purpose that we should decide whether these Conceptions are absolutely ultimate principles of thought, or whether, on the contrary, they can be further resolved into other Fundamental Ideas. We need not now suppose it determined whether or not Number is a mere modification of the Idea of Time, and Force a mere modification of the Idea of Cause: for however this may be, our Conception of Number is the foundation of Arithmetic, and our Conception of Force is the foundation of Mechanics. It is to be observed also that in our classification, each Science may involve, not only the Ideas or Conceptions which are placed opposite to it in the list, but also all which *precede* it. Thus Formal Astronomy involves not only the Conception of Motion, but also those which are the foundation of Arithmetic and Geometry. In like manner,

Physical Astronomy employs the Sciences of Statics and Dynamics, and thus, rests on their foundations ; and they, in turn, depend upon the Ideas of Space and of Time, as well as of Cause.

3. We may further observe, that this arrangement of Sciences according to the Fundamental Ideas which they involve, points out the transition from those parts of human knowledge which have been included in our History and Philosophy, to other regions of speculation into which we have not entered. We have repeatedly found ourselves upon the borders of inquiries of a psychological, or moral, or theological nature. Thus the History of Physiology\* led us to the consideration of Life, Sensation, and Volition ; and at these Ideas we stopped, that we might not transgress the boundaries of our subject as then predetermined. It is plain that the pursuit of such conceptions and their consequences, would lead us to the sciences (if we are allowed to call them sciences) which contemplate not only animal, but human principles of action, to Anthropology and Psychology. In other ways, too, the Ideas which we have examined, although manifestly the foundations of sciences such as we have here treated of, also plainly pointed to speculations of a different order ; thus the Idea of a Final Cause is an indispensable guide in Biology, as we have seen ; but the conception of Design as directing the order of nature, once admitted, soon carries us to higher contemplations. Again, the Class of Palætiological Sciences which we were in the *History* led to construct, although we there admitted only one example of the Class, namely Geology, does in reality include many vast lines of research ; as the history and causes of the diffusion of plants and animals, the history of languages, arts, and

\* *Hist. Ind. Sci.* B. xvii. c. v. sect. 2.

consequently of civilization. Along with these researches, comes the question how far these histories point backwards to a natural or a supernatural origin; and the Idea of a First Cause is thus brought under our consideration. Finally, it is not difficult to see that as the Physical Sciences have their peculiar governing Ideas, which support and shape them, so the Moral and Political Sciences also must similarly have their fundamental and formative Ideas, the source of universal and certain truths, each of their proper kind. But to follow out the traces of this analogy, and to verify the existence of those Fundamental Ideas in Morals and Politics, is a task quite out of the sphere of the work in which we are here engaged.

4. We may now place before the reader our Classification of the Sciences. I have added to the list of Sciences, a few not belonging to our present subject, that the nature of the transition by which we are to extend our philosophy into a wider and higher region may be in some measure perceived.

We may observe that the term *Physics*, when confined to a peculiar class of Sciences, is usually understood to exclude the Mechanical Sciences on the one side, and Chemistry on the other; and thus embraces the Secondary Mechanical and Analytico-Mechanical Sciences. But the adjective *Physical* applied to any science and opposed to *Formal*, as in Astronomy and Optics, implies those speculations in which we consider not only the Laws of Phenomena but their Causes; and generally, as in those cases, their Mechanical Causes.

| Fundamental Ideas or Conceptions. | Sciences.                                | Classification.                              |
|-----------------------------------|------------------------------------------|----------------------------------------------|
| Space . . .                       | Geometry . . .                           | Pure Mathematical Sciences.                  |
| Time . . .                        | . . . . .                                |                                              |
| Number . . .                      | Arithmetic . . .                         |                                              |
| Sign . . .                        | Algebra . . .                            |                                              |
| Limit . . .                       | Differentials . . .                      | Pure Motional Sciences                       |
| Motion . . .                      | Pure Mechanism . . .                     |                                              |
|                                   | Formal Astronomy                         |                                              |
| Cause                             |                                          |                                              |
| Force                             | Statics . . .                            | Mechanical Sciences.                         |
| Matter . . .                      | Dynamics . . .                           |                                              |
| Inertia . . .                     | Hydrostatics . . .                       |                                              |
| Fluid Pressure . . .              | Hydrodynamics . . .                      |                                              |
|                                   | Physical Astronomy                       |                                              |
| Outness                           |                                          |                                              |
| Medium of Sensation               | Acoustics . . .                          | Secondary Mechanical Sciences.<br>(Physics.) |
| Intensity of Qualities            | Formal Optics . . .                      |                                              |
| Scales of Qualities . . .         | Physical Optics . . .                    |                                              |
|                                   | Thermotics . . .                         |                                              |
|                                   | Atmology . . .                           | Analytico-Mechanical Sciences.<br>(Physics.) |
| Polarity . . .                    | Electricity . . .                        |                                              |
|                                   | Magnetism . . .                          |                                              |
|                                   | Galvanism . . .                          |                                              |
| Element (Composition)             |                                          |                                              |
| Chemical Affinity                 |                                          |                                              |
| Substance (Atoms)                 | Chemistry . . .                          | Analytical Science.                          |
| Symmetry . . .                    | Crystallography . . .                    | Analytico-Classificatory Sciences.           |
| Likeness . . .                    | Systematic Mineralogy                    |                                              |
| Degrees of Likeness               | Systematic Botany                        | Classificatory Sciences.                     |
|                                   | Systematic Zoology                       |                                              |
|                                   | Comparative Anatomy                      |                                              |
| Natural Affinity . . .            |                                          |                                              |
| (Vital Powers)                    |                                          |                                              |
| Assimilation                      |                                          |                                              |
| Irritability                      |                                          |                                              |
| (Organization) . . .              | Biology . . .                            | Organical Sciences.                          |
| Final Cause                       |                                          |                                              |
| Instinct                          |                                          |                                              |
| Emotion . . .                     | Psychology                               |                                              |
| Thought                           |                                          |                                              |
| Historical Causation              | Geology . . .                            | Pætiological Sciences.                       |
|                                   | Distribution of Plants and Animals . . . |                                              |
|                                   | Glossology . . .                         |                                              |
|                                   | Ethnography                              |                                              |
| First Cause . . .                 | Natural Theology.                        |                                              |

In the next Book, we shall trace the opinions of some of the most eminent writers, respecting the sources of our knowledge of nature and the rules which may aid us in seeking it. For the knowledge of a true Scientific Method is a science resembling other sciences; and the ideas and views which it involves have been in some measure gradually developed into clearness and certainty by successive attempts. We may, therefore, acquire a more confident persuasion of the right direction of our path, by seeing how far it coincides with that which has been pointed out, with more or less distinctness, by many of the most sagacious and vigorous intellects who have bestowed their attention upon this inquiry.

THE  
Saturn  
Laws.

N. v.  
their Sal  
of the d  
Planet.

|                                                      |                                              |                                                     |                                |
|------------------------------------------------------|----------------------------------------------|-----------------------------------------------------|--------------------------------|
| Colours pass-<br>ing through<br>water, glass,<br>&c. | Colours<br>seen by<br>diff. transp. in<br>Tw | Colours in<br>diff. transp. in<br>substances.<br>Rh | Optical<br>instrument".<br>Ca. |
| Measures.<br>&c.                                     | rainbow,<br>&c.                              |                                                     |                                |

Facts of  
First Law of Phenomena. The effects take  
By the *Idea of a Medium* Light an  
First Facts. The common and obvious Pha

(2.)

THE SATELLITES of Jupiter and Saturn revolve according to Kepler's Laws.

By Mechanics.

Newton Jupiter and Saturn attract their Satellites inversely as the square of the distance, and the Sun attracts Planets and Satellites alike.



Buttons.  
Young.

These Laws of these Phænomena were  
first discovered till Theory had in-  
terpreted them.

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Young.  
resnel.

\* By interf.  
of undul<sup>ns</sup>.  
from all  
parts.  
\* Fraun-  
hofer.

\* By interf.  
of rays  
from striæ.  
\* Young.

*Newton's Scale of Colours.*

*Fits of Rays.*  
Newton

|                          |                          |
|--------------------------|--------------------------|
| * By interf.             | By interf.               |
| of undul <sup>ns</sup> . | of undul <sup>ns</sup> . |
| from two                 | from two                 |
| surfaces.                | surfaces.                |
| * Young.                 | * Young.                 |

Young.  
resnel.

Colours of Fringes, Gratings, Striæ, thick Plates,  
thin Plates, &c. are produced by interference of  
undulations; length of undulation being different  
for different colours.

different for different colours.

## BOOK XII.

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### *REVIEW OF OPINIONS ON THE NATURE OF KNOWLEDGE AND THE METHODS OF SEEKING IT.*

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#### CHAPTER I.

#### INTRODUCTION.

By the examination of the elements of human thought in which we have been engaged, and by a consideration of the history of the most clear and certain parts of our knowledge, we have been led to certain doctrines respecting the progress of that exact and systematic knowledge which we call Science; and these doctrines we have endeavoured to lay before the reader in the preceding Book. The questions on which we have thus ventured to pronounce have had a strong interest for man, from the earliest period of his intellectual progress, and have been the subjects of lively discussion and bold speculation in every age. We conceive that in the doctrines to which our researches have conducted us, we have a far better hope that we possess a body of permanent truths, than the earlier essays on the same subjects could furnish. For we have not taken our examples of knowledge at hazard, as earlier speculators did, and were almost compelled to do; but have drawn our materials from the vast store of unquestioned truths which modern science offers to us: and we have formed our judgment concerning the nature and progress of

knowledge by considering what such science is, and how it has reached its present condition. But though we have thus pursued our speculations concerning knowledge with advantages which earlier writers did not possess, it is still both interesting and instructive for us to regard the opinions upon this subject which have been delivered by the philosophers of past times. It is especially interesting to see some of the truths which we have endeavoured to expound, gradually dawning in men's minds, and assuming the clear and permanent form in which we can now contemplate them. I shall therefore, in this Book, pass in review many of the opinions of the writers of various ages concerning the mode by which man best acquires the truest knowledge; and I shall endeavour, as we proceed, to appreciate the real value of such judgments, and their place in the progress of sound philosophy.

In this estimate of the opinions of others, I shall be guided by those general doctrines which I have, as I trust, established in the preceding part of this work. And without attempting here to give any summary of these doctrines, I may remark that there are two main principles by which speculations on such subjects in all ages are connected and related to each other; namely, the opposition of *Ideas* and *Sensations*, and the distinction of *practical* and *speculative* knowledge. The opposition of Ideas and Sensations is exhibited to us in the antithesis of Theory and Fact, which are necessarily considered as distinct and of opposite natures, and yet necessarily identical, and constituting Science by their identity. In like manner, although practical knowledge is in substance identical with speculative, (for all knowledge is speculation,) there is a distinction between the two in their history, and in the subjects by which they are exemplified, which distinction is quite essential in

judging of the philosophical views of the ancients. The alternatives of identity and diversity, in these two antitheses,—the successive separation, opposition, and reunion of principles which thus arise,—have produced, (as they may easily be imagined capable of doing,) a long and varied series of systems concerning the nature of knowledge ; among which we shall have to guide our course by the aid of the views already presented.

I am far from undertaking, or wishing, to review the whole series of opinions which thus comes under our view ; and I do not even attempt to examine all the principal authors who have written on such subjects. I merely wish to select some of the most considerable forms which such opinions have assumed, and to point out in some measure the progress of truth from age to age. In doing this, I can only endeavour to seize some of the most prominent features of each time and of each step ; and I must pass rapidly from classical antiquity to those which we have called the dark ages, and from them to modern times. At each of these periods the modifications of opinion, and the speculations with which they were connected, formed a vast and tangled maze, into the byways of which our plan does not allow us to enter. We shall esteem ourselves but too fortunate, if we can discover the single track by which ancient led to modern philosophy.

I must also repeat that my survey of philosophical writers is here confined to this one point,—their opinions on the nature of knowledge and the method of science. I with some effort avoid entering upon other parts of the philosophy of those of whom I speak ; I knowingly pass by those portions of their speculations which are in many cases the most interesting and celebrated ;—their opinions concerning the human soul, the Divine governor of the world, the foundations or leading

doctrines of politics, religion, and general philosophy. I am desirous that my reader should bear this in mind, since he must otherwise be offended with the scanty and partial view which I give in this place of the philosophers whom I enumerate.

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## CHAPTER II.

### PLATO.

THERE would be small advantage in beginning our examination earlier than the period of the Socratic School at Athens; for although the spirit of inquiry on such subjects had awoke in Greece at an earlier period, and although the peculiar aptitude of the Grecian mind for such researches had shown itself repeatedly in subtle distinctions and acute reasonings, all the positive results of these early efforts were contained in a more definite form in the reasonings of the Platonic age. Anterior to that time, the Greeks did not possess plain and familiar examples of exact knowledge, such as the truths of Arithmetic, Geometry, Astronomy, and Optics, became in the school of Plato; nor were the antitheses of which we spoke above, so distinctly and fully unfolded as we find them in Plato's works.

The question which hinges upon one of these antitheses, occupies a prominent place in several of the Platonic dialogues;—namely, whether our knowledge be obtained by means of Sensation or of Ideas. One of the doctrines which Plato most earnestly inculcated upon his countrymen was, that we do not *know* concerning sensible objects, but concerning ideas. The first attempts of the Greeks at metaphysical analysis had given rise to a school which maintained that material objects are the

only realities. In opposition to this, arose another school, which taught that material objects have no permanent reality, but are ever waxing and waning, constantly changing their substance. "And hence," as Aristotle says\*, "arose the doctrine of ideas which the Platonists held. For they assented to the opinion of Heraclitus, that all sensible objects are in a constant state of flux. So that if there is to be any knowledge and science, it must be concerning some permanent natures, different from the sensible natures of objects; for there can be no permanent science respecting that which is perpetually changing. It happened that Socrates turned his speculations to the moral virtues, and was the first philosopher who endeavoured to give universal definitions of such matters. He wished to reason systematically, and therefore he tried to establish definitions, for definitions are the basis of systematic reasoning. There are two things which may justly be looked upon as steps in philosophy due to Socrates; inductive reasonings, and universal definitions;—both of them steps which belong to the foundations of science. Socrates, however, did not make universals, or definitions separable from the objects; but his followers separated them, and these essences they termed *Ideas*." And the same account is given by other writers†. "Some existences are sensible, some intelligible: and according to Plato, they who wish to understand the principles of things, must first separate the ideas from the things, such as the ideas of Similarity, Unity, Number, Magnitude, Position, Motion: second, that we must assume an absolute Fair, Good, Just, and the like: third, that we must consider the ideas of relation, as Knowledge, Power: recollecting that the things which we perceive have this or that appellation applied to them because

\* *Metaph.*, XII. 4.† *Diog. Laert. Vit. Plat.*

they partake of this or that Idea; those things being *just* which participate in the idea of The Just, those being *beautiful*, which contain the idea of The Beautiful." And many of the arguments by which this doctrine was maintained are to be found in the Platonic dialogues. Thus the opinion that true knowledge consists in sensation, which had been asserted by Protagoras and others, is refuted in the *Theætetus*: and we may add, so victoriously refuted, that the arguments there put forth have ever since exercised a strong influence upon the speculative world. It may be remarked that in the minds of Plato and of those who have since pursued the same paths of speculation, the interest of such discussions as those we are now referring to, was by no means limited to their bearing upon mere theory; but was closely connected with those great questions of morals which have always a practical import. Those who asserted that the only foundation of knowledge was sensation, asserted also that the only foundation of virtue was the desire of pleasure. And in Plato, the metaphysical part of the disquisitions concerning knowledge in general, though independent in its principles, always seems to be subordinate in its purpose to the questions concerning the knowledge of our duty.

Since Plato thus looked upon the Ideas which were involved in each department of knowledge as forming its only essential part, it was natural that he should look upon the study of Ideas as the true mode of pursuing knowledge. This he himself describes in the *Philebus*\*. "The best way of arriving at truth is not very difficult to point out, but most hard to pursue. All the arts which have ever been discovered, were revealed in this manner. It is a gift of the gods to man, which, as I conceive, they sent down by some Prometheus, in a blaze of light; and

\* T. II. p. 16, c, d. ed. Bekker, t. v. p. 437.

the ancients, more clear-sighted than we, and less removed from the gods, handed down this traditionary doctrine: that whatever is said to be, comes of One and of Many, and comprehends in itself the Finite and the Infinite in coalition (being One Kind, and consisting of Infinite Individuals). And this being the state of things, we must, in each case, endeavour to seize the One Idea (the idea of the Kind) as the chief point; for we shall find that it is there. And when we have seized this one thing, we may then consider how it comprehends in itself two, or three, or any other number; and, again, examine each of these ramifications separately; till at last we perceive, not only that One is at the same time One and Many, but also how many. And when we have thus filled up the interval between the Infinite and the One, we may consider that we have done with each one. The gods then, as I have said, taught us by tradition thus to contemplate, and to learn, and to teach one another. But the philosophers of the present day seize upon the One, at hazard, too soon or too late, and then immediately snatch at the Infinite; but the intermediate steps escape them, by which the subject is subdivided, so that it can be the subject of logical exposition and discussion."

It would seem that what the author here describes as the most perfect form of exposition, is that which refers each object to its place in a classification containing a complete series of subordinations, and which gives a definition of each class. We have repeatedly remarked that, in sciences of classification, each new definition which gives a tenable and distinct separation of classes is an important advance in our knowledge; but that such definitions are rather the last than the first step in each advance. In the progress of real knowledge, these definitions are always the results of a laborious study of



individual cases, and are never arrived at by a pure effort of thought, which is what Plato appears to have imagined as the true mode of philosophizing. And still less do the advances of other sciences consist in seizing at once upon the highest generality, and filling in afterwards all the intermediate steps between that and the special instances. On the contrary, as we have seen, the ascents from particular to general are all successive; and each step of this ascent requires time, and labour, and a patient examination of actual facts and objects.

It would, of course, be absurd to blame Plato for having inadequate views of the nature of progressive knowledge, at the time when knowledge could hardly be said to have begun its progress. But we already find in his speculations, as appears in the passages just quoted from his writings, several points brought into view which will require our continued attention as we proceed. In overlooking the necessity of a gradual and successive advance from the less general to the more general truths, Plato shared in a dimness of vision which prevailed among philosophers to the time of Francis Bacon. In thinking too slightly of the study of actual nature, he manifested a bias from which the human intellect freed itself in the vigorous struggles which terminated the dark ages. In pointing out that all knowledge implies a unity of what we observe as manifold, which unity is given by the mind, Plato taught a lesson which has of late been too obscurely acknowledged, the recoil by which men repaired their long neglect of facts having carried them for a while so far as to think that facts were the whole of our knowledge. And in analyzing this principle of Unity, by which we thus connect sensible things, into various Ideas, such as Number, Magnitude, Position, Motion, he made a highly impor-

tant step, which it has been the business of philosophers in succeeding times to complete and to follow out.

But the efficacy of Plato's speculations in their bearing upon physical science, and upon theory in general, was much weakened by the confusion of practical with theoretical knowledge, which arose from the ethical propensities of the Socratic school. In the Platonic Dialogues, Art and Science are constantly spoken of indiscriminately. The skill possessed by the Painter, the Architect, the Shoemaker, is considered as a just example of human science, no less than the knowledge which the geometer or the astronomer possesses of the theoretical truths with which he is conversant. Not only so; but traditionary and mythological tales, mystical imaginations and fantastical etymologies, are mixed up, as no less choice ingredients, with the most acute logical analyses, and the most exact conduct of metaphysical controversies. There is no distinction made between the knowledge possessed by the theoretical psychologist and the physician, the philosophical teacher of morals and the legislator or the administrator of law. This, indeed, is the less to be wondered at, since even in our own time the same confusion is very commonly made by persons not otherwise ignorant or uncultured.

On the other hand, we may remark finally, that Plato's admiration of Ideas was not a barren imagination, even so far as regarded physical science. For, as we have seen\*, he had a very important share in the introduction of the theory of epicycles, having been the first to propose to astronomers in a distinct form, the problem of which that theory was the solution; namely, "to explain the celestial phenomena by the combination of equable circular motions." This demand of an ideal hypothesis which should exactly express the phenomena

\* *Hist. Ind. Sci.*, B. III. c. ii.

(as well as they could then be observed), and from which, by the interposition of suitable steps, all special cases might be deduced, falls in well with those views respecting the proper mode of seeking knowledge which we have quoted from the *Philebus*. And the Idea which could thus represent and replace all the particular Facts, being not only sought but found, we may readily suppose that the philosopher was, by this event, strongly confirmed in his persuasion that such an Idea was indeed what the inquirer ought to seek. In this conviction all his genuine followers up to modern times have participated; and thus, though they have avoided the error of those who hold that facts alone are valuable as the elements of our knowledge, they have frequently run into the opposite error of too much despising and neglecting facts, and of thinking that the business of the inquirer after truth was only a profound and constant contemplation of the conceptions of his own mind. But of this hereafter.

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### CHAPTER III.

#### ARISTOTLE.

THE views of Aristotle with regard to the foundations of human knowledge are very different from those of his tutor Plato, and are even by himself put in opposition to them. He dissents altogether from the Platonic doctrine that Ideas are the true materials of our knowledge; and after giving, respecting the origin of this doctrine, the account which we quoted in the last chapter, he goes on to reason against it. "Thus," he says\*, "they devised Ideas of all things which are spoken of as universals:

\* *Metaph.* XII. 4.

much as if any one having to count a number of objects, should think that he could not do it while they were few, and should expect to count them by making them more numerous. For the kinds of things are almost more numerous than the special sensible objects, by seeking the causes of which they were led to their Ideas." He then goes on to urge several other reasons against the assumption of Ideas and the use of them in philosophical researches.

Aristotle himself establishes his doctrines by trains of reasoning. But reasoning must proceed from certain First Principles; and the question then arises, Whence are these First Principles obtained? To this he replies, that they are the result of *Experience*, and he even employs the same technical expression by which we at this day describe the process of collecting these principles from observed facts;—that they are obtained by *Induction*. I have already quoted passages in which this statement is made\*. "The way of reasoning," he says†, "is the same in philosophy, and in any art or science: we must collect the *facts* (τὰ ὑπαρχόντα), and the things to which the facts happen, and must have as large a supply of these as possible, and then we must examine them according to the terms of our syllogisms." . . . "There are peculiar principles in each science; and in each case these principles must be obtained from *experience*. Thus astronomical observation supplies the principles of astronomical science. For the phenomena being rightly taken, the demonstrations of astronomy were discovered; and the same is the case with any other Art or Science. So that if the facts in each case be taken, it is our business to construct the demonstrations. For if in *our natural history* (κατὰ τὴν ἱστορίαν) we have omitted none of the facts and properties which belong to the subject,

\* *Hist. Ind. Sci.*, B. 1. c. iii. sect. 2.    † *Analyt. Prior.*, 1. 30.

we shall learn what we can demonstrate and what we cannot." And, again\*, "It is manifest that if any sensation be wanting, there must be some knowledge wanting, which we are thus prevented from having. For we acquire knowledge either *by Induction* (ἐπαγωγή) or by Demonstration: and Demonstration is from universals, but Induction from particulars. It is impossible to have universal theoretical propositions except by Induction: and we cannot make inductions without having sensation; for sensation has to do with particulars."

It is easy to show that Aristotle uses the term *Induction*, as we use it, to express the process of collecting a general proposition from particular cases in which it is exemplified. Thus in a passage which we have already quoted†, he says, "Induction, and Syllogism from Induction, is when we attribute one extreme term to the middle by means of the other." The import of this technical phraseology will further appear by the example which he gives: "We find that several animals which are deficient in bile are longlived, as man, the horse, the mule; hence we infer that *all* animals which are deficient in bile are longlived."

We may observe, however, that both Aristotle's notion of induction, and many other parts of his philosophy, are obscure and imperfect, in consequence of his refusing to contemplate ideas as something distinct from sensation. It thus happens that he always assumes the ideas which enter into his proposition as *given*; and considers it as the philosopher's business to determine whether such propositions are true or not: whereas the most important feature in induction is, as we have said, the *introduction* of a new idea, and not its employment when once introduced. That the mind in this manner

\* *Analyt. Post.*, I. 18.

† *Anal. Pri.*, II. 23, περὶ τῆς ἐπαγωγῆς.

gives unity to that which is manifold,—that we are thus led to speculative principles which have an evidence higher than any others,—and that a peculiar sagacity in some men seizes upon the conceptions by which the facts may be bound into true propositions,—are doctrines which form no essential part of the philosophy of the Stagirite, although such views are sometimes recognized, more or less clearly, in his expressions. Thus he says\*, “There can be no knowledge when the sensation does not continue in the mind. For this purpose, it is necessary both to perceive, and to have some *unity* in the mind; (*αἰσθανομένοις ἔχειν ἓν τι ἐν τῇ ψυχῇ*) and many such perceptions having taken place, some difference is then perceived: and from the remembrance of these arises Reason. Thus from Sensation comes Memory, and from Memory of the same thing often repeated comes Experience: for many acts of Memory make up one Experience. And from Experience, or from any Universal Notion which takes a permanent place in the mind,—from the *unity in the manifold*, the same some one thing being found in many facts,—springs the first principle of Art and of Science; of Art, if it be employed about production; of Science, if about existence.”

I will add to this, Aristotle's notice of *Sagacity*; since, although little or no further reference is made to this quality in his philosophy, the passage fixes our attention upon an important step in the formation of knowledge. “Sagacity,” (*ἀγχινοια*) he says†, “is a hitting by guess (*ἐντροχία τις*) upon the middle term (the conception common to two cases) in an inappreciable time. As for example, if any one seeing that the bright side of the moon is always towards the sun, suddenly perceives why this is; namely, because the moon shines by the light of the sun:—or if he sees a person talking with a

\* *Anal. Post.*, II. 19.† *Ib.*, I. 34.

rich man, he guesses that he is borrowing money ;—or conjectures that two persons are friends, because they are enemies of the same person.”—To consider only the first of these examples ;—the conception here introduced, that of a body shining by the light which another casts upon it, is not contained in the observed facts, but introduced by the mind. It is, in short, that conception which, in the act of induction, the mind superadds to the phenomena as they are presented by the senses : and to invent such appropriate conceptions, such “eustochies,” is, indeed, the precise office of inductive sagacity.

At the end of this work (the *Later Analytics*) Aristotle ascribes our knowledge of principles to Intellect, (*νοῦς*) or, as it appears necessary to translate the word, *Intuition*\*. “Since, of our intellectual habits by which we aim at truth, some are always true, but some admit of being false, as Opinion and Reasoning, but Science and Intuition are always true ; and since there is nothing which is more certain than Science except Intuition ; and since Principles are better known to us than the Deductions from them ; and since all Science is connected by reasoning, we cannot have Science respecting Principles. Considering this then, and that the beginning of Demonstration cannot be Demonstration, nor the beginning of Science, Science ; and since, as we have said, there is no other kind of truth, Intuition must be the beginning of Science.”

What is here said, is, no doubt, in accordance with the doctrines which we have endeavoured to establish respecting the nature of Science, if by this *Intuition* we understand that contemplation of certain Fundamental Ideas, which is the basis of all rigorous knowledge. But notwithstanding this apparent approximation, Aristotle was far from having an habitual and practical possession

\* *Anal. Post.*, II. 19.

of the principles which he thus touches upon. He did not, in reality, construct his philosophy by giving Unity to that which was manifold, or by seeking in Intuition principles which might be the basis of Demonstration; nor did he collect, in each subject, fundamental propositions by an induction of particulars. He rather endeavoured to divide than to unite; he employed himself, not in combining facts, but in analyzing notions; and the criterion to which he referred his analysis was, not the facts of our experience, but our habits of language. Thus his opinions rested, not upon sound inductions, gathered in each case from the phenomena by means of appropriate Ideas; but upon the loose and vague generalizations which are implied in the common use of speech.

Yet Aristotle was so far consistent with his own doctrine of the derivation of knowledge from experience, that he made in almost every province of human knowledge, a vast collection of such special facts as the experience of his time supplied. These collections are almost unrivalled, even to the present day, especially in Natural History; in other departments, when to the facts we must add the right Inductive Idea, in order to obtain truth, we find little of value in the Aristotelic works. But in those parts which refer to Natural History, we find not only an immense and varied collection of facts and observations, but a sagacity and acuteness in classification which it is impossible not to admire. This indeed appears to have been the most eminent faculty in Aristotle's mind.

The influence of Aristotle in succeeding ages will come under our notice shortly.

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## CHAPTER IV.

## THE LATER GREEKS.

THUS while Plato was disposed to seek the essence of our knowledge in Ideas alone, Aristotle, slighting this source of truth, looked to Experience as the beginning of Science; and he attempted to obtain, by division and deduction, all that Experience did not immediately supply. And thus, with these two great names, began that struggle of opposite opinions which has ever since that time agitated the speculative world, as men have urged the claims of Ideas or of Experience to our respect, and as alternately each of these elements of knowledge has been elevated above its due place, while the other has been unduly depressed. We shall see the successive turns of this balanced struggle in the remaining portions of this review.

But we may observe that practically the influence of Plato predominated rather than that of Aristotle, in the remaining part of the history of ancient philosophy. It was, indeed, an habitual subject of dispute among men of letters, whether the sources of true knowledge are to be found in the Senses or in the Mind; the Epicureans taking one side of this alternative, and the Academics another, while the Stoics in a certain manner included both elements in their view. But none of these sects showed their persuasion that the materials of knowledge were to be found in the domain of Sense, by seeking them there. No one appears to have thought of following the example of Aristotle, and gathering together a store of observed facts. We may except, perhaps, assertions belonging to some provinces of Natural History, which were collected by various writers: but in these, the mixed character of the statements, the want of dis-

crimination in the estimate of evidence, the credulity and love of the marvellous which the authors for the most part displayed, showed that instead of improving upon the example of Aristotle, they were wandering further and further from the path of real knowledge. And while they thus collected, with so little judgment, such statements as offered themselves, it hardly appears to have occurred to any one to enlarge the stores of observation by the aid of experiment; and to learn what the laws of nature were, by trying what were their results in particular cases. They used no instruments for obtaining an insight into the constitution of the universe, except logical distinctions and discussions; and proceeded as if the phenomena familiar to their predecessors must contain all that was needed as a basis for natural philosophy. By thus contenting themselves with the facts which the earlier philosophers had contemplated, they were led also to confine themselves to the ideas which those philosophers had put forth. For all the most remarkable alternatives of hypothesis, so far as they could be constructed with a slight and common knowledge of phenomena, had been promulgated by the acute and profound thinkers who gave the first impulse to philosophy: and it was not given to man to add much to the original inventions of *their* minds till he had undergone anew a long discipline of observation, and of thought employed upon observation. Thus the later authors of the Greek Schools became little better than commentators on the earlier; and the common places with which the different schools carried on their debates,—the constantly recurring argument, with its known attendant answer,—the distinctions drawn finer and finer and leading to nothing,—render the speculations of those times a *scholastic* philosophy, in the same sense in which we employ the term when we speak of the labours of

the middle ages. It will be understood that I now refer to that which is here my subject, the opinions concerning our knowledge of nature, and the methods in use for the purpose of obtaining such knowledge. Whether the moral speculations of the ancient world were of the same stationary kind, going their round in a limited circle, like their metaphysics and physics, must be considered on some other occasion.

As a specimen of the later Greek reasonings on physical philosophy, I may take a passage from Galen's Commentary on the Treatise of Hippocrates, *On the Elements*. "What, then," he asks\*, "is the method of discovering these Elements? To me it seems there can be no other than that which was introduced by Hippocrates. For we must reason first, considering if an Element be a thing which is one, according to its idea; (ἐν τι τὴν ἰδέαν;) and next, if many and various and dissimilar, how many, and of what kind they are, and how related by their association. Now that the First Element is not one only, comprising both our bodies and other things, Hippocrates shows. For if man were one Element only, he could not fall sick; for there would be nothing which could derange his health, if he were of one Element only." We have seen, in the *History of Science*, that Galen is one of the greatest names in ancient Physiology: but when he makes the attempt to pass at one step from the most familiar facts to the ultimate constitution of the universe, it is not wonderful that his reasonings are of no real value or import.

Before we quit the ancients we may observe some peculiarities in the Roman disciples of the Greek philosophy, which may be worthy our notice.

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\* Lib. i. c. ii.

## CHAPTER V.

## THE ROMANS.

THE Romans had no philosophy but that which they borrowed from the Greeks; and what they thus received, they hardly made entirely their own. The vast and profound question of which we have been speaking, the relation between Existence and our Knowledge of what exists, they never appear to have fathomed, even so far as to discern how wide and deep it is. In the development of the ideas by which nature is to be understood, they went no further than their Greek masters had gone, nor indeed was more to be looked for. And in the practical habit of accumulating observed facts as materials for knowledge, they were much less discriminating and more credulous than their Greek predecessors. The descent from Aristotle to Pliny, in the judiciousness of the authors and the value of their collections of facts, is immense.

Since the Romans were thus servile followers of their Greek teachers, and little acquainted with any example of new truths collected from the world around them, it was not to be expected that they could have any just conception of that long and magnificent ascent from one set of truths to others of higher order and wider compass, which the history of science began to exhibit when the human mind recovered its progressive habits. Yet some dim presentiment of the splendid career thus destined for the intellect of man appears from time to time to have arisen in their minds. Perhaps the circumstance which most powerfully contributed to suggest this vision, was the vast intellectual progress which they were themselves conscious of having made, through the introduction of the Greek philosophy; and to this may be added, per-

haps, some other features of national character. Their temper was too stubborn to acquiesce in the absolute authority of the Greek philosophy, although their minds were not inventive enough to establish a rival by its side. And the wonderful progress of their political power had given them a hope in the progress of man which the Greeks never possessed. The Roman, as he believed the fortune of his State to be destined for eternity, believed also in the immortal destiny and endless advance of that Intellectual Republic of which he had been admitted a denizen.

It is easy to find examples of such feelings as I have endeavoured to describe. The enthusiasm with which Lucretius and Virgil speak of physical knowledge, manifestly arises in a great measure from the delight which they had felt in becoming acquainted with the Greek theories.

Me vero primum dulces ante omnia musæ  
Quarum sacra fero ingenti percussus amore,  
Accipiant, cœlique vias et sidera monstrent,  
Defectus solis varios, Lunæque labores!  
Felix qui potuit rerum cognoscere causas!

Ovid\* expresses a similar feeling.

Felices animos quibus hæc cognoscere primis  
Inque domos superas scandere cura fuit! . . .  
Admovere oculis distantia sidera nostris  
Ætheraque ingenio supposuere suo.  
Sic petitur cœlum: non ut ferat Ossam Olympus  
Summaque Pelicæ sidera tanget apex.

And from the whole tenour of these and similar passages, it is evident that the intellectual pleasure which arises from our first introduction to a beautiful physical theory had a main share in producing this enthusiasm at the contemplation of the victories of science; although undoubtedly the moral philosophy, which was never sepa-

\* L. 1., *Fast.*

rated from the natural philosophy, and the triumph over superstitious fears which a knowledge of nature was supposed to furnish, added warmth to the feeling of exultation.

We may trace a similar impression in the ardent expressions which Pliny\* makes use of in speaking of the early astronomers, and which we have quoted in the *History*. "Great men! elevated above the common standard of human nature, by discovering the laws which celestial occurrences obey, and by freeing the wretched mind of man from the fears which eclipses inspired."

This exulting contemplation of what science had done, naturally led the mind to an anticipation of further achievements still to be performed. Expressions of this feeling occur in Seneca, and are of the most remarkable kind, as the following example will show†.

"Why do we wonder that comets, so rare a phenomena, have not yet had their laws assigned?—that we should know so little of their beginning and their end, when their recurrence is at wide intervals? It is not yet fifteen hundred years since Greece,

Stellis numeros et nomina fecit,

reckoned the stars, and gave them names. There are still many nations which are acquainted with the heavens by sight only; which do not yet know why the moon disappears, why she is eclipsed. It is but lately that among us philosophy has reduced these matters to a certainty. The day shall come when the course of time and the labour of a maturer age shall bring to light what is yet concealed. One generation, even if it devoted itself to the skies, is not enough for researches so extensive. How then can it be so, when we divide this scanty allowance of years into no equal shares between our studies and our vices? These things then must be explained by a

\* *Hist. Nat.* i. 75.

† *Quæst. Nat.*, vii. 25.

long succession of inquiries. We have but just begun to know how arise the morning and evening appearances, the stations, the progressions, and the retrogradations of the fixed stars which put themselves in our way;—which appearing perpetually in another and another place compel us to be curious. Some one will hereafter demonstrate in what region the comets wander; why they move so far asunder from the rest; of what size and nature they are. Let us be content with what we have discovered: let posterity contribute its share to truth." Again he adds\* in the same strain. "Let us not wonder that what lies so deep is brought out so slowly. How many animals have become known for the first time in this age! And the members of future generations shall know many of which we are ignorant. Many things are reserved for ages to come, when our memory shall have passed away. The world would be a small thing indeed, if it did not contain matter of inquiry for all the world. Eleusis reserves something for the second visit of the worshipper. *So too Nature does not at once disclose all HER mysteries.* We think ourselves initiated; we are but in the vestibule. The arcana are not thrown open without distinction and without reserve. This age will see some things; that which comes after us, others."

While we admire the happy coincidence of these conjectures with the soundest views which the history of science teaches us, we must not forget that they are merely conjectures, suggested by very vague impressions, and associated with very scanty conceptions of the laws of nature. Seneca's *Natural Questions*, from which the above extract is taken, contains a series of dissertations on various subjects of Natural Philosophy; as Meteors, Rainbows, Lightning, Springs, Rivers, Snow, Hail, Rain, Wind, Earthquakes and Comets. In the whole of these

\* *Quæst. Nat.*, vii. 30, 31.

dissertations, the statements are loose, and the explanations of little or no value. Perhaps it may be worth our while to notice a case in which he refers to an observation of his own, although his conclusion from it be erroneous. He is arguing\* against the opinion that Springs arise from the water which falls in rain. "In the first place," he says, "I, a very diligent digger in my vineyard, affirm that no rain is so heavy as to moisten the earth to the depth of more than ten feet. All the moisture is consumed in this outer crust, and descends not to the lower part." We have here something of the nature of an experiment; and indeed, as we may readily conceive, the instinct which impels man to seek truth by experiment can never be altogether extinguished. Seneca's experiment was deprived of its value by the indistinctness of his ideas, which led him to rest in the crude conception of the water being "consumed" in the superficial crust of the earth.

It is unnecessary to pursue further the reasonings of the Romans on such subjects, and we now proceed to the ages which succeeded the fall of their empire.

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## CHAPTER VI. .

### THE SCHOOLMEN OF THE MIDDLE AGES.

IN the *History of the Sciences* I have devoted a Book to the state of Science in the middle ages, and have endeavoured to analyze the intellectual defects of that period. Among the characteristic features of the human mind during those times, I have noticed Indistinctness of Ideas, a Commentatorial Spirit, Mysticism, and Dogmatism. The account there given of this portion of the

\* *Quæst. Nat.*, III. 7.



history of man belongs, in reality, rather to the present work than to the History of Progressive Science. For, as we have there remarked, theoretical Science was, during the period of which we speak, almost entirely stationary; and the investigation of the causes of such a state of things may be considered as a part of that review, in which we are now engaged, of the vicissitudes of man's acquaintance with the methods of discovery. But when we offered to the world a history of science, to leave so large a chasm unexplained, would have made the series of events seem defective and broken; and the survey of the Middle Ages was therefore inserted. I would beg to refer to that portion of the former work the reader who wishes for information in addition to what is here given.

The Indistinctness of Ideas and the Commentatorial Disposition of those ages have already been here brought under our notice. Viewed with reference to the opposition between Experience and Ideas, on which point, as we have said, the succession of opinions in a great measure turns, it is clear that the commentatorial method belongs to the ideal side of the question: for the commentator seeks for such knowledge as he values, by analyzing and illustrating what his author has said; and, content with this material of speculation, does not desire to add to it new stores of experience and observation. And with regard to the two other features in the character which we gave to those ages, we may observe that Dogmatism demands for philosophical theories the submission of mind, due to those revealed religious doctrines which are to guide our conduct and direct our hopes: while Mysticism elevates ideas into realities, and offers them to us as the objects of our religious regard. Thus the Mysticism of the middle ages and their Dogmatism alike arose from not discriminating the offices of

theoretical and practical philosophy. Mysticism claimed for ideas the dignity and reality of principles of moral action and religious hope: Dogmatism imposed theoretical opinions respecting speculative points with the imperative tone of rules of conduct and faith.

If, however, the opposite claims of theory and practice interfered with the progress of science by the confusion they thus occasioned, they did so far more by drawing men away altogether from mere physical speculations. The Christian religion, with its precepts, its hopes, and its promises, became the leading subject of men's thoughts; and the great active truths thus revealed, and the duties thus enjoined, made all inquiries of mere curiosity appear frivolous and unworthy of man. The Fathers of the Church sometimes philosophized ill; but far more commonly they were too intent upon the great lessons which they had to teach, respecting man's situation in the eyes of his Heavenly Master, to philosophize at all respecting things remote from the business of life and of no importance in man's spiritual concerns.

Yet man has his intellectual as well as his spiritual wants. He has faculties which demand systems and reasons, as well as precepts and promises. The Christian doctor, who knew so much more than the heathen philosopher respecting the Creator and Governor of the universe, was not long content to know or to teach less, respecting the universe itself. While it was still maintained that Theology was the only really important study, Theology was so extended and so fashioned as to include all other knowledge: and after no long time, the Fathers of the Church themselves became the authors of systems of universal knowledge.

But when this happened, the commentatorial spirit was still in its full vigour. The learned Christians could not, any more than the later Greeks or the Romans,

devise, by the mere force of their own invention, new systems, full, comprehensive, and connected, like those of the heroic age of philosophy. The same mental tendencies which led men to look for speculative coherence and completeness in the view of the universe, led them also to admire and dwell upon the splendid and acute speculations of the Greeks. They were content to find, in these immortal works, the answers to the questions which their curiosity prompted; and to seek what further satisfaction they might require, in analyzing and unfolding the doctrines promulgated by those great masters of knowledge. Thus the Christian doctors became, as to general philosophy, commentators upon the ancient Greek teachers.

Among these, they selected Aristotle as their peculiar object of admiration and study. The vast store, both of opinions and facts, which his works contain, his acute distinctions, his cogent reasons in some portions of his speculations, his symmetrical systems in almost all, naturally commended him to the minds of subtle and curious men. We may add that Plato, who taught men to contemplate Ideas separate from Things, was not so well fitted for general acceptance as Aristotle, who rejected this separation. For although the due apprehension of this opposition of ideas and sensations is a necessary step in the progress of true philosophy, it requires a clearer view and a more balanced mind than the common herd of students possess; and Aristotle, who evaded the necessary perplexities in which this antithesis involves us, appeared, to the temper of those times, the easier and the plainer guide of the two.

The Doctors of the middle ages having thus adopted Aristotle as their master in philosophy, we shall not be surprized to find them declaring, after him, that experience is the source of our knowledge of the visible world. But

though, like the Greeks, they thus talked of experiment, like the Greeks, they showed little disposition to discover the laws of nature by observation of facts. This barren and formal recognition of experience or sensation as one source of knowledge, not being illustrated by a practical study of nature, and by real theoretical truths obtained by such a study, remained ever vague, wavering, and empty. Such a mere acknowledgement cannot, in any times, ancient or modern, be considered as indicating a just apprehension of the true basis and nature of science.

In imperfectly perceiving how, and how far, experience is the source of our knowledge of the external world, the teachers of the middle ages were in the dark ; but so, on this subject, have been almost all the writers of all ages, with the exception of those who in recent times have had their minds enlightened by contemplating philosophically the modern progress of science. The opinions of the doctors of the middle ages on such subjects generally had those of Aristotle for their basis ; but the subject was often still further analyzed and systematized, with an acute and methodical skill hardly inferior to that of Aristotle himself.

The Stagirite, in the beginning of his *Physics*, had made the following remarks. "In all bodies of doctrine which involve principles, causes, or elements, Science and Knowledge arise from the knowledge of these ; (for we then consider ourselves to *know* respecting any subject, when we know its first cause, its first principles, its ultimate elements.) It is evident, therefore, that in seeking a knowledge of nature, we must first know what are its principles. But the course of our knowledge is, from the things which are better known and more manifest to us, to the things which are more certain and evident in nature. For those things which are most evident in truth, are not most evident to us.

[And consequently we must advance from things obscure in nature, but manifest to us, towards the things which are really in nature more clear and certain.] The things which are first obvious and apparent to us are complex; and from these we obtain, by analysis, principles and elements. We must proceed from universals to particulars. For the whole is better known to our senses than the parts, and for the same reason, the universal better known than the particular. And thus words signify things in a large and indiscriminate way, which is afterwards analyzed by definition; as we see that the children at first call all men *father*, and all women *mother*, but afterwards learn to distinguish."

There are various assertions contained in this extract which came to be considered as standard maxims, and which occur constantly in the writers of the middle ages. Such are, for instance, the maxim, "Verè scire est per causas scire;" the remark, that compounds are known to us before their parts, and the illustration from the expressions used by children. Of the mode in which this subject was treated by the schoolmen, we may judge by looking at passages of Thomas Aquinas which treat of the subject of the human understanding. In the *Summa Theologiæ*, the eighty-fifth Question is *On the manner and order of understanding*, which subject he considers in eight Articles; and these must, even now, be looked upon as exhibiting many of the most important and interesting points of the subject. They are, *First*, Whether our understanding understands by abstracting ideas (*species*) from appearances; *Second*, Whether intelligible species abstracted from appearances are related to our understanding as that *which* we understand, or that *by which* we understand; *Third*, Whether our understanding does naturally understand universals first; *Fourth*, Whether our understanding can understand many things at once;

*Fifth*, Whether our understanding understands by compounding and dividing; *Sixth*, Whether the understanding can err; *Seventh*, Whether one person can understand the same thing better than another; *Eighth*, Whether our understanding understands the indivisible sooner than the divisible. And in the discussion of the last point, for example, reference is made to the passage of Aristotle which we have already quoted. "It may seem," he says, "that we understand the indivisible before the divisible; for *the Philosopher* says that we understand and know by knowing principles and elements; but indivisibles are the principles and elements of divisible things. But to this we may reply, that in our receiving of science, principles and elements are not always first; for sometimes from the sensible effects we go on to the knowledge of intelligible principles and causes." We see that both the objection and the answer are drawn from Aristotle.

We find the same close imitation of Aristotle in Albertus Magnus, who, like Aquinas, flourished in the thirteenth century. Albertus, indeed, wrote treatises corresponding to almost all those of the Stagirite, and was called the *Ape of Aristotle*. In the beginning of his *Physics*, he says, "Knowledge does not always begin from that which is first according to the nature of things, but from that of which the knowledge is easiest. For the human intellect, on account of its relation to the senses (*propter reflexionam quam habet ad sensum*), collects science from the senses; and thus it is easier for our knowledge to begin from that which we can apprehend by sense, imagination, and intellect, than from that which we apprehend by intellect alone." We see that he has somewhat systematized what he has borrowed.

This disposition to dwell upon and systematize the leading doctrines of metaphysics assumed a more definite and permanent shape in the opposition of the

Realists and Nominalists. The opposition involved in this controversy is, in fact, that fundamental antithesis of Sense and Ideas about which philosophy has always been engaged; and of which we have marked the manifestation in Plato and Aristotle. The question, What is the object of our thoughts when we reason concerning the external world? must occur to all speculative minds: and the difficulties of the answer are manifest. We must reply, either that our own Ideas, or that Sensible Things, are the elements of our knowledge of nature. And then the scruples again occur,—how we have any *general* knowledge if our thoughts are fixed on particular objects; and, on the other hand,—how we can attain to any *true* knowledge of nature by contemplating ideas which are not identical with objects in nature. The two opposite opinions maintained on this subject were, on the one side,—that our general propositions refer to objects which are *real*, though divested of the peculiarities of individuals; and, on the other side,—that in such propositions, individuals are not represented by any reality, but bound together by a *name*. These two views were held by the Realists and Nominalists respectively: and thus the Realist manifested the adherence to Ideas, and the Nominalist the adherence to the impressions of Sense, which have always existed as opposite yet correlative tendencies in man.

The Realists were the prevailing sect in the Scholastic times: for example, both Thomas Aquinas and Duns Scotus, the *Angelical* and the *Subtle* Doctor, held this opinion, although opposed to each other in many of their leading doctrines on other subjects. And as the Nominalist, fixing his attention upon sensible objects, is obliged to consider what is the *principle of generalization*, in order that the possibility of any general proposition may be conceivable; so on the other hand, the Realist, begin-

ning with the contemplation of universal ideas, is compelled to ask what is the *principle of individuation*, in order that he may comprehend the application of general propositions in each particular instance. This inquiry concerning the principle of individuation was accordingly a problem which occupied all the leading minds among the Schoolmen\*. It will be apparent from what has been said, that it is only one of the many forms of the fundamental antithesis of the Ideas and the Senses, which we have constantly before us in this review.

The recognition of the derivation of our knowledge, in part at least, from Experience, though always loose and incomplete, appears often to be independent of the Peripatetic traditions. Thus Richard of St. Victor, a writer of contemplative theology in the twelfth century, says†, that "there are three sources of knowledge, experience, reason, faith. Some things we prove by experiment, others we collect by reasoning, the certainty of others we hold by believing. And with regard to temporal matters, we obtain our knowledge by actual experience; the other guides belong to divine knowledge." Richard also propounds a division of human knowledge which is clearly not derived directly from the ancients, and which shows that considerable attention must have been paid to such speculations. He begins by laying down clearly and broadly the distinction, which, as we have seen, is of primary importance, between *practice* and *theory*. *Practice*, he says, includes seven mechanical arts; those of the clothier, the armourer, the navigator, the hunter, the physician, and the player. *Theory* is threefold, divine, natural, doctrinal; and is thus divided into Theology, Physics, and Mathematics. *Mathematics*, he adds, treats

\* See the opinion of Aquinas, in Degerando, *Hist. Com. des Syst.* iv. 499; of Duns Scotus, *ib.*, iv. 523.

† *Liber Excerptionum*, lib. i. c. i.



of the invisible *forms* of visible things. We have seen that by many profound thinkers this word *forms* has been selected as best fitted to describe those relations of things which are the subject of mathematics. Again, Physics discovers causes from their effects and effects from their causes. It would not be easy at the present day to give a better account of the object of physical science. But Richard of St. Victor makes this account still more remarkably judicious, by the examples to which he alludes; which are earthquakes, the tides, the virtues of plants, the instincts of animals, the classification of minerals, plants and reptiles.

Unde tremor terris, quâ vi maria alta tumescant,  
Herbarum vires, animos irasque ferarum,  
Omne genus fruticum, lapidum quoque, reptiliumque.

He further adds\*, "Physical science ascends from effects to causes, and descends again from causes to effects." This declaration Francis Bacon himself might have adopted. It is true, that Richard would probably have been little able to produce any clear and definite instances of knowledge, in which this ascent and descent were exemplified; but still the statement, even considered as a mere conjectural thought, contains a portion of that sagacity and comprehensive power which we admire so much in Bacon.

Richard of St. Victor, who lived in the twelfth century, thus exhibits more vigour and independence of speculative power than Thomas Aquinas, Albertus Magnus, and Duns Scotus, in the thirteenth. In the interval, about the end of the twelfth century, the writings of Aristotle had become generally known in the West; and had been elevated into the standard of philosophical doctrine, by the divines mentioned above, who felt a reverent sympathy with the systematizing and subtle

\* *Tr. Ex. Lib. i. c. vii.*

spirit of the Stagirite as soon as it was made manifest to them. These doctors, following the example of their great forerunner, reduced every part of human knowledge to a systematic form; the systems which they thus framed were presented to men's minds as the only true philosophy, and dissent from them was no longer considered to be blameless. It was an offence against religion as well as reason to reject the truth, and the truth could be but one. In this manner arose that claim which the Doctors of the Church put forth to control men's opinions upon all subjects, and which we have spoken of in the *History of Science* as the Dogmatism of the Middle Ages. There is no difficulty in giving examples of this characteristic. We may take for instance a Statute of the University of Paris, occasioned by a Bull of Pope John XXI., in which it is enacted, "that no Master or Bachelor of any faculty, shall presume to read lectures upon any author in a private room, on account of the many perils which may arise therefrom; but shall read in public places, where all may resort, and may faithfully report what is there taught; excepting only books of Grammar and Logic, in which there can be no presumption." And certain errors of Brescain are condemned in a Rescript\* of the papal Legate Odo, with the following expressions: "Whereas, as we have been informed, certain Logical professors treating of Theology in their disputations, and Theologians treating of Logic, contrary to the command of the law are not afraid to mix and confound the lots of the Lord's heritage; we exhort and admonish your University, all and singular, that they be content with the landmarks of the Sciences and Faculties which our Fathers have fixed; and that having due fear of the curse pronounced in the law against him who removeth his neighbour's landmark,

\* Tenneman, VIII. 461.

you hold such sober wisdom according to the Apostles, that ye may by no means incur the blame of innovation or presumption."

The account which, in the *History of Science*, I gave of Dogmatism as a characteristic of the middle ages, has been indignantly rejected by a very pleasing modern writer, who has, with great feeling and great diligence, brought into view the merits and beauties of those times, termed by him *Ages of Faith*. He urges\* that religious authority was never claimed for physical science: and he quotes from Thomas Aquinas, a passage in which the author protests against the practice of confounding opinions of philosophy with doctrines of faith. We might quote in return the Rescript† of Stephen, bishop of Paris, in which he declares that there can be but one truth, and rejects the distinction of things being true according to philosophy and not according to the Catholic faith; and it might be added, that among the errors condemned in this document are some of Thomas Aquinas himself. We might further observe, that if no physical doctrines were condemned in the times of which we now speak, this was because, on such subjects, no new opinions were promulgated, and not because opinion was free. As soon as new opinions, even on physical subjects, attracted general notice, they were prohibited by authority, as we see in the case of Galileo‡.

\* *Mores Catholici, or Ages of Faith*, VIII p. 247.

† Tenneman, VIII. 460.

‡ If there were any doubt on this subject, we might refer to the writers who afterwards questioned the supremacy of Aristotle, and who with one voice assert that an infallible authority had been claimed for him. Thus Laurentius Valla: "Quo minus ferendi sunt recentes Peripatetici, qui nullius sectæ hominibus interdiciunt libertate ab Aristotele dissentiendi, quasi sophos hic, non philosophus." *Pref. in Dial.* (Tenneman, ix. 29.) So Ludovicus Vives: "Sunt ex philosophis et ex theologis qui non solem quo Aristoteles pervenit extremum esse aiunt na-

But this disinclination to recognize philosophy as independent of religion, and this disposition to find in new theories, even in physical ones, something contrary to religion or scripture, are, it would seem, very natural tendencies of theologians; and it would be unjust to assert that these propensities were confined to the periods when the authority of papal Rome was highest; or that the spirit which has in a great degree controlled and removed such habits was introduced by the Reformation of religion in the sixteenth century. We must trace to other causes, the clear and general recognition of Philosophy, as distinct from Theology, and independent of her authority. In the earlier ages of the Church, indeed, this separation had been acknowledged. St. Augustin says, "A Christian should beware how he speaks on questions of natural philosophy, as if they were doctrines of Holy Scripture; for an infidel who should hear him deliver absurdities could not avoid laughing. Thus the Christian would be confused, and the infidel but little edified; for the infidel would conclude that our authors really entertained these extravagant opinions, and therefore they would despise them, to their own eternal ruin. Therefore the opinions of philosophers should never be proposed as dogmas of faith, or rejected as contrary to faith, when it is not certain that they are so." These words are quoted with

turæ, sed quâ pervenit eam rectissimam esse omnium et certissimam in natura viam." (Tenneman, ix. 43.) We might urge too, the evasions practised by philosophical Reformers, through fear of the dogmatism to which they had to submit; for example, the protestation of Telesius at the end of the Proem to his work, *De Rerum Natura*: "Nec tamen, si quid eorum quæ nobis posita sunt, sacris literis, Catholicæ ecclesiæ decretis non cohæreat, tenendum id, quin penitus rejiciendum asseveramus contendimusque. Neque enim humana modo ratio quævis, sed ipse etiam sensus illis posthabendus, et si illis non congruat, abnegandus omnino et ipse etiam est sensus."

approbation by Thomas Aquinas, and it is said\*, are cited in the same manner in every encyclopedical work of the middle ages. This warning of genuine wisdom was afterwards rejected, as we have seen; and it is only in modern times that its value has again been fully recognized. And this improvement we must ascribe, mainly, to the progress of physical science. For a great body of undeniable truths on physical subjects being accumulated, such as had no reference to nor connexion with the truths of religion, and yet such as possessed a strong interest for most men's minds, it was impossible longer to deny that there were wide provinces of knowledge which were not included in the dominions of Theology, and over which she had no authority. In the fifteenth and sixteenth centuries, the fundamental doctrines of mechanics, hydrostatics, optics, magnetics, chemistry, were established and promulgated; and along with them, a vast train of consequences, attractive to the mind by the ideal relations which they exhibited, and striking to the senses by the power which they gave man over nature. Here was a region in which philosophy felt herself entitled and impelled to assert her independence. From this region, there is a gradation of subjects in which philosophy advances more and more towards the peculiar domain of religion; and at some intermediate points there have been, and probably will always be, conflicts respecting the boundary line of the two fields of speculation. For the limit is vague and obscure, and appears to fluctuate and shift with the progress of time and knowledge.

Our business at present is not with the whole extent and limits of philosophy, but with the progress of physical science more particularly, and the methods by

\* *Ages of Faith*, viii. 247: to the author of which I am obliged for this quotation.

which it may be attained: and we are endeavouring to trace historically the views which have prevailed respecting such methods, at various periods of man's intellectual progress. Among the most conspicuous of the revolutions which opinions on this subject have undergone, is the transition from an implicit trust in the internal powers of man's mind to a professed dependence upon external observation; and from an unbounded reverence for the wisdom of the past, to a fervid expectation of change and improvement. The origin and progress of this disposition of mind;—the introduction of a state of things in which men not only obtained a body of indestructible truths from experience, and increased it from generation to generation, but professedly, and we may say, ostentatiously, declared such to be the source of their knowledge, and such their hopes of its destined career;—the rise, in short, of Experimental Philosophy, not only as a habit, but as a Philosophy of Experience, is what we must now endeavour to exhibit.

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## CHAPTER VII.

### THE INNOVATORS OF THE MIDDLE AGES.

1. *General Remarks.*—IN the rise of Experimental Philosophy, understanding the term in the way just now stated, two features have already been alluded to: the disposition to cast off the prevalent reverence for the opinions and methods of preceding teachers with an eager expectation of some vast advantage to be derived from a change; and the belief that this improvement must be sought by drawing our knowledge from external observation rather than from mere intellectual efforts;—*the Insurrection against Authority, and the Appeal*

to *Experience*. These two movements were closely connected; but they may easily be distinguished, and in fact, persons were very prominent in the former part of the task, who had no comprehension of the latter principle, from which alone the change derives its value. There were many Malcontents who had not the temper, talent or knowledge, which fitted them to be Reformers.

The authority which was questioned, in the struggles of which we speak, was that of the Scholastic System, the combination of Philosophy with Theology; of which Aristotle, presented in the form and manner which the Doctors of the Church had imposed upon him, is to be considered the representative. When there was demanded of men a submission of the mind, such as this system claimed, the natural love of freedom in man's bosom, and the speculative tendencies of his intellect, rose in rebellion, from time to time, against the ruling oppression. We find in all periods of the scholastic ages examples of this disposition of man to resist overstrained authority; the tendency being mostly, however, combined with a want of solid thought, and showing itself in extravagant pretensions and fantastical systems put forwards by the insurgents. We have pointed out one such opponent\* of the established systems, even among the Arabian schoolmen, a more servile race than ever the Europeans were. We may here notice more especially an extraordinary character who appeared in the thirteenth century, and who may be considered as belonging to the Prelude of the Reform in Philosophy, although he had no share in the Reform itself.

2. *Raymond Lully*.—Raymond Lully is perhaps traditionally best known as an Alchemist, of which art he appears to have been a cultivator. But this was only one of the many impulses of a spirit ardently thirsty

\* Algazel. See *Hist. Ind. Sci.*, B. iv. c. i.

of knowledge and novelty. He had\*, in his youth, been a man of pleasure, but was driven by a sudden shock of feeling to resolve on a complete change of life. He plunged into solitude, endeavoured to still the remorse of his conscience by prayer and penance, and soon had his soul possessed by visions which he conceived were vouchsafed him. In the feeling of religious enthusiasm thus excited, he resolved to devote his life to the diffusion of Christian truth among Heathens and Mahomedans. For this purpose, at the age of thirty he betook himself to the study of Grammar, and of the Arabic language. He breathed earnest supplications for an illumination from above; and these were answered by his receiving from heaven, as his admirers declare, his *Ars Magna*, by which he was able without labour or effort to learn and apply all knowledge. The real state of the case is, that he put himself in opposition to the established systems, and propounded a New Art, from which he promised the most wonderful results; but that his Art really is merely a mode of combining ideal conceptions without any reference to real sources of knowledge, or any possibility of real advantage. In a Treatise addressed, in A. D. 1310, to King Philip of France, entitled *Liber Lamentationis Duodecim Principiorum Philosophiæ contra Averroistas*, Lully introduces Philosophy, accompanied by her twelve Principles, (Matter, Form, Generation, &c.) uttering loud complaints against the prevailing system of doctrine; and represents her as presenting to the king a petition that she may be upheld and restored by her favourite, the Author. His *Tabula Generalis ad omnes Scientias applicabilis* was begun the 15th September, 1292, in the Harbour of Tunis, and finished in 1293, at Naples. In order to frame an Art of thus tabulating all existing sciences,

\* Tenneman, viii. 830.



and indeed all possible knowledge, he divides into various classes the conceptions with which he has to deal. The first class contains nine *Absolute Conceptions*: Goodness, Greatness, Duration, Power, Wisdom, Will, Virtue, Truth, Majesty. The second class has nine *Relative Conceptions*: Difference, Identity, Contrariety, Beginning, Middle, End, Majority, Equality, Minority. The third class contains nine *Questions*: Whether? What? Whence? Why? How great? How circumstanced? When? Where? and How? The fourth class contains the nine *Most General Subjects*: God, Angel, Heaven, Man, *Imaginativum*, *Sensitivum*, *Vegetativum*, *Elementativum*, *Instrumentativum*. Then come nine *Prædicaments*, nine *Moral Qualities*, and so on. These conceptions are arranged in the compartments of certain concentric moveable circles, and give various combinations by means of triangles and other figures, and thus propositions are constructed.

It must be clear at once, that real knowledge, which is the union of facts and ideas, can never result from this machinery for shifting about, joining and disjoining, empty conceptions. This, and all similar schemes, go upon the supposition that the logical combinations of notions do of themselves compose knowledge; and that really existing things may be arrived at by a successive system of derivation from our most general ideas. It is imagined that by distributing the nomenclature of abstract ideas according to the place which they can hold in our propositions, and by combining them according to certain conditions, we may obtain formulæ including all possible truths, and thus fabricate a science in which all sciences are contained. We thus obtain the means of talking and writing upon all subjects, without the trouble of thinking: the revolutions of the emblematical figures are substituted for the operations of the

mind. Both exertion of thought, and knowledge of facts, become superfluous. And this reflection, adds an intelligent author\*, explains the enormous number of books which Lully is said to have written; for he might have written those even during his sleep, by the aid of a moving power which should keep his machine in motion. Having once devised this invention for manufacturing science, Lully varied it in a thousand ways, and followed it into a variety of developments. Besides Synoptical Tables, he employs Genealogical Trees, each of which he dignifies with the name of the Tree of Science. The only requisite for the application of his System was a certain agreement in the numbers of the classes into which different subjects were distributed; and as this symmetry does not really exist in the operations of our thoughts, some violence was done to the natural distinction and subordination of conceptions, in order to fit them for the use of the System.

Thus Lully, while he professed to teach an Art which was to shed new light upon every part of science, was in fact employed in a pedantic and trifling repetition of known truths or truisms; and while he complained of the errors of existing methods, he proposed in their place one which was far more empty, barren, and worthless, than the customary processes of human thought. Yet his method is spoken of† with some praise by Leibnitz, who indeed rather delighted in the region of ideas and words, than in the world of realities. But Francis Bacon speaks far otherwise and more justly on this subject‡. “It is not to be omitted that some men, swollen with emptiness rather than knowledge, have laboured to produce a certain Method, not deserving the name of a legitimate Method, since it is rather

\* Degerando, iv. 535.

† *Opera*, v. 16.

‡ *Works*, vii. 296.

a method of imposture: which yet is doubtless highly grateful to certain would-be philosophers. This method scatters about certain little drops of science in such a manner that a smatterer may make a perverse and ostentatious use of them with a certain show of learning. Such was the Art of Lully, which consisted of nothing but a mass and heap of the words of each science; with the intention that he who can readily produce the words of any science shall be supposed to know the science itself. Such collections are like a rag shop, where you find a patch of everything, but nothing which is of any value."

3. *Roger Bacon*.—We now come to a philosopher of a very different character, who was impelled to declare his dissent from the reigning philosophy by the abundance of his knowledge, and by his clear apprehension of the mode in which real knowledge had been acquired and must be increased.

Roger Bacon was born in 1214, near Ilchester, in Somersetshire, of an old family. In his youth he was a student at Oxford, and made extraordinary progress in all branches of learning. He then went to the University of Paris, as was at that time the custom of learned Englishmen, and there received the degree of Doctor of Theology. At the persuasion of Robert Grosstête, bishop of Lincoln, he entered the brotherhood of Franciscans in Oxford, and gave himself up to study with extraordinary fervour. He was termed by his brother monks *Doctor Mirabilis*. We know from his own works, as well as from the traditions concerning him, that he possessed an intimate acquaintance with all the science of his time which could be acquired from books; and that he had made many remarkable advances by means of his own experimental labours. He was acquainted with Arabic, as well as with the other languages com-

mon in his time. In the title of his works, we find the whole range of science and philosophy, Mathematics and Mechanics, Optics, Astronomy, Geography, Chronology, Chemistry, Magic, Music, Medicine, Grammar, Logic, Metaphysics, Ethics, and Theology; and judging from those which are published, these works are full of sound and exact knowledge. He is, with good reason, supposed to have discovered, or to have had some knowledge of, several of the most remarkable inventions which were made generally known soon afterwards; as gunpowder, lenses, burning specula, telescopes, clocks, the correction of the calendar, and the explanation of the rainbow.

Thus possessing, in the acquirements and habits of his own mind, abundant examples of the nature of knowledge and of the process of invention, Roger Bacon felt also a deep interest in the growth and progress of science, a spirit of inquiry respecting the causes which produced or prevented its advance, and a fervent hope and trust in its future destinies; and these feelings impelled him to speculate worthily and wisely respecting a Reform of the Method of Philosophizing. The manuscripts of his works have existed for nearly six hundred years in many of the libraries of Europe, and especially in those of England; and for a long period the very imperfect portions of them which were generally known, left the character and attainments of the author shrouded in a kind of mysterious obscurity. About a century ago, however, his *Opus Majus* was published\* by Dr. S. Jebb, principally from a manuscript in the library of Trinity College, Dublin; and this contained most or all of the

\* *Fratri Rogeri Bacon Ordinis Minorum Opus Majus ad Clementem Quartum, Pontificem Romanum, ex MS. Codice Dubliniensi cum aliis quibusdam collato nunc primum edidit S. Jebb, M.D. Londini, 1733.*

separate works which were previously known to the public, along with others still more peculiar and characteristic. We are thus able to judge of Roger Bacon's knowledge and of his views, and they are in every way well worthy our attention.

The *Opus Majus* is addressed to Pope Clement the Fourth, whom Bacon had known when he was legate in England as Cardinal-bishop of Sabina, and who admired the talents of the monk, and pitied him for the persecutions to which he was exposed. On his elevation to the papal chair, this account of Bacon's labours and views was sent, at the earnest request of the pontiff. Besides the *Opus Majus*, he wrote two others, the *Opus Minus* and *Opus Tertium*; which were also sent to the pope, as the author says\*, "on account of the danger of roads, and the possible loss of the work." These works still exist unpublished, in the Cottonian and other libraries.

The *Opus Majus* is a work equally wonderful with regard to its general scheme, and to the special treatises with which the outlines of the plan are filled up. The professed object of the work is to urge the necessity of a reform in the mode of philosophizing, to set forth the reasons why knowledge had not made a greater progress, to draw back attention to the sources of knowledge which had been unwisely neglected, to discover other sources which were yet almost untouched, and to animate men in the undertaking, by a prospect of the vast advantages which it offered. In the development of this plan, all the leading portions of science are expounded in the most complete shape which they had at that time assumed; and improvements of a very wide and striking kind are proposed in some of the principal of these departments. Even if the work had had no

\* *Opus Majus*, Præf.

leading purpose, it would have been highly valuable as a treasure of the most solid knowledge and soundest speculations of the time; even if it had contained no such details, it would have been a work most remarkable for its general views and scope. It may be considered as, at the same time, the *Encyclopedia* and the *Novum Organon* of the thirteenth century.

Since this work is thus so important in the history of Inductive Philosophy I shall give, in a note, a view\* of its divisions and contents. But I must now endeavour to point out more especially the way in which the various principles, which the reform of scientific method involved, are here brought into view.

\* Contents of Roger Bacon's *Opus Majus*.

Part I. On the four causes of human ignorance:—Authority, Custom, Popular Opinion, and the Pride of supposed Knowledge.

Part II. On the source of perfect wisdom in the Sacred Scripture.

Part III. On the Usefulness of Grammar.

Part IV. On the Usefulness of Mathematics.

(1.) The necessity of Mathematics in Human Things (published separately as the *Specula Mathematica*).

(2.) The necessity of Mathematics in Divine Things.—1°. This study has occupied holy men: 2°. Geography: 3°. Chronology: 4°. Cycles; the Golden Number, &c.: 5°. Natural Phenomena, as the Rainbow: 6°. Arithmetic: 7°. Music.

(3.) The Necessity of Mathematics in Ecclesiastical Things. 1°. The Certification of Faith: 2°. The Correction of the Calendar.

(4.) The Necessity of Mathematics in the State.—1°. Of Climates: 2°. Hydrography: 3°. Geography: 4°. Astrology.

Part V. On Perspective (published separately as *Perspectiva*).

(1.) The organs of vision.

(2.) Vision in straight lines.

(3.) Vision reflected and refracted.

(4.) De multiplicatione specierum (on the propagation of the impressions of light, heat, &c.)

Part VI. On Experimental Science.

One of the first points to be noticed for this purpose, is the resistance to authority; and at the stage of philosophical history with which we here have to do, this means resistance to the authority of Aristotle, as adopted and interpreted by the Doctors of the Schools. Bacon's work\* is divided into Six Parts; and of these Parts, the First is, Of the four universal Causes of all Human Ignorance. The causes thus enumerated† are:—the force of unworthy authority;—traditionary habit;—the imperfection of the undisciplined senses;—and the disposition to conceal our ignorance and to make an ostentatious show of our knowledge. These influences involve every man, occupy every condition. They prevent our obtaining the most useful and large and fair doctrines of wisdom, the secrets of all sciences and arts. He then proceeds to argue, from the testimony of philosophers themselves, that the authority of antiquity, and especially of Aristotle, is not infallible. "We find‡ their books full of doubts, obscurities, and perplexities. They scarce agree with each other in one empty question or one worthless sophism, or one operation of science, as one man agrees with another in the practical operations of medicine, surgery, and the like arts of Secular men. Indeed," he adds, "not only the philosophers, but the saints have fallen into errors which they have afterwards retracted," and this he instances in Augustin, Jerome, and others. He gives an admirable sketch of the progress of philosophy from the Ionic School to Aristotle; of whom he speaks with great applause. "Yet," he adds§, "those who came after him corrected him in some things, and added many things to his works, and shall go on adding to the end of the world." Aristotle, he adds, is now called peculiarly|| the Philoso-

\* *Op. Maj.*, p. 1.

† *Ib.*, p. 2.

‡ *Ib.*, p. 10.

§ *Op. Maj.*, p. 36.

|| *Autonomicé.*

pher, "yet there was a time when his philosophy was silent and unregarded, either on account of the rarity of copies of his works, or their difficulty, or from envy; till after the time of Mahomet, when Avicenna and Averroes, and others, recalled this philosophy into the full light of exposition. And although the Logic, and some other works were translated by Boethius from the Greek, yet the philosophy of Aristotle first received a quick increase among the Latins at the time of Michael Scot; who, in the year of our Lord 1230, appeared, bringing with him portions of the books of Aristotle on Natural Philosophy and Mathematics. And yet a small part only of the works of this author is translated, and a still smaller part is in the hands of common students." He adds further\* (in the Third Part of the *Opus Majus*, which is a Dissertation on Language) that the translations which are current of these writings, are very bad and imperfect. With these views, he is moved to express himself somewhat impatiently† respecting these works: "If I had," he says, "power over the works of Aristotle, I would have them all burnt; for it is only a loss of time to study in them, and a course of error, and a multiplication of ignorance beyond expression." "The common herd of students," he says, "with their heads, have no principle by which they can be excited to any worthy employment; and hence they mope and

\* *Op. Maj.*, p. 46.

† See *Pref.* to Jebb's edition. The passages there quoted, however, are not extracts from the *Opus Majus*, but (apparently) from the *Opus Minus* (*MS. Cott. Tib. c. 5.*) "Si haberem potestatem supra libros Aristotelis, ego facerem omnes cremari; quia non est nisi temporis amissio studere in illis, et causa erroris, et multiplicatio ignorantie ultra id quod valeat explicari. . . . Vulgus studentum cum capitibus suis non habet unde excitetur ad aliquid dignum, et ideo languet et asinuat circa male translata, et tempus et studium amittit in omnibus et expensas."



and make asses of themselves over their bad translations, and lose their time, and trouble, and money."

The remedies which he recommends for these evils, are, in the first place, the study of that only perfect wisdom which is to be found in the sacred Scripture\*, in the next place, the study of mathematics and the use of experiment†. By the aid of these methods, Bacon anticipates the most splendid progress for human knowledge. He takes up the strain of hope and confidence which we have noticed as so peculiar in the Roman writers; and quotes some of the passages of Seneca which we adduced in illustration of this:—that the attempts in science were at first rude and imperfect, and were afterwards improved;—that the day will come, when what is still unknown shall be brought to light by the progress of time and the labours of a longer period;—that one age does not suffice for inquiries so wide and various;—that the people of future times shall know many things unknown to us;—and that the time shall arrive when posterity will wonder that we overlooked what was so obvious. Bacon himself adds anticipations more peculiarly in the spirit of his own time. "We have seen," he says, at the end of the work, "how Aristotle, by the ways which wisdom teaches, could give to Alexander the empire of the world. And this the Church ought to take into consideration against the infidels and rebels, that there may be a sparing of Christian blood, and especially on account of the troubles that shall come to pass in the days of Antichrist; which by the grace of God, it would be easy to obviate, if prelates and princes would encourage study, and join in searching out the secrets of nature and art."

It may not be improper to observe here that this belief in the appointed progress of knowledge, is not

\* Part II.

† Parts IV., V. and VI.

combined with any overweening belief in the unbounded and independent power of the human intellect. On the contrary, one of the lessons which Bacon draws from the state and prospects of knowledge, is the duty of faith and humility. "To him," he says\*, "who denies the truth of the faith because he is unable to understand it, I will propose in reply the course of nature, and as we have seen it in examples." And after giving some instances, he adds, "These, and the like, ought to move men and to excite them to the reception of divine truths. For if, in the vilest objects of creation, truths are found, before which the inward pride of man must bow, and believe though it cannot understand, how much more should man humble his mind before the glorious truths of God!" He had before said†: "Man is incapable of perfect wisdom in this life; it is hard for him to ascend towards perfection, easy to glide downwards to falsehoods and vanities: let him then not boast of his wisdom, or extol his knowledge. What he knows is little and worthless, in respect of that which he believes without knowing; and still less, in respect of that which he is ignorant of. He is mad who thinks highly of his wisdom; he most mad, who exhibits it as something to be wondered at." He adds, as another reason for humility, that he has proved by trial, he could teach in one year, to a poor boy, the marrow of all that the most diligent person could acquire in forty years' laborious and expensive study.

To proceed somewhat more in detail with regard to Roger Bacon's views of a Reform in Scientific Inquiry, we may observe that by making Mathematics and Experiment the two great points of his recommendation, he directed his improvement to the two essential parts of all knowledge, Ideas and Facts, and thus took the course

\* *Op. Maj.*, p. 476.

† *Ib.*, p. 15.

which the most enlightened philosophy would have suggested. He did not urge the prosecution of experiment, to the comparative neglect of the existing mathematical sciences and conceptions; a fault which there is some ground for ascribing to his great namesake and successor Francis Bacon: still less did he content himself with a mere protest against the authority of the schools, and a vague demand for change, which was almost all that was done by those who put themselves forward as reformers in the intermediate time. Roger Bacon holds his way steadily between the two poles of human knowledge; which, as we have seen, it is far from easy to do. "There are two modes of knowing," says he\*; "by argument, and by experiment. Argument concludes a question; but it does not make us feel certain, or acquiesce in the contemplation of truth, except the truth be also found to be so by experience." It is not easy to express more decidedly the clearly seen union of exact conceptions with certain facts, which, as we have explained, constitutes real knowledge.

One large division of the *Opus Majus* is "On the Usefulness of Mathematics," which is shown by a copious enumeration of existing branches of knowledge, as Chronology, Geography, the Calendar, and (in a separate Part) Optics. There is a chapter†, "in which it is proved by reason, that all science requires mathematics." And the arguments which are used to establish this doctrine, show a most just appreciation of the office of mathematics in science. They are such as follows:—

\* *Op. Maj.* p. 445, see also p. 448. "Scientiæ aliæ sciunt sua principia invenire per experimenta, sed conclusiones per argumenta facta ex principiis inventis. Si vero debeant habere experientiam conclusionum suarum particularem et completam, tunc oportet quod habeant per adjutorium istius scientiæ nobilis, (experimentalis.)"

† *Op. Maj.*, p. 60.

That other sciences use examples taken from mathematics as the most evident:—That mathematical knowledge is, as it were, innate in us, on which point he refers to the well known dialogue of Plato, as quoted by Cicero:—That this science, being the easiest, offers the best introduction to the more difficult:—That in mathematics, things as known to us are identical with things as known to nature:—That we can here entirely avoid doubt and error, and obtain certainty and truth:—That mathematics is prior to other sciences in nature, because it takes cognizance of quantity, which is apprehended by intuition, (*intuitu intellectus*.) “Moreover,” he adds\*, “there have been found famous men, as Robert, bishop of Lincoln, and Brother Adam Marshman, (de Marisco) and many others, who by the power of mathematics have been able to explain the causes of things; as may be seen in the writings of these men, for instance, concerning the Rainbow and Comets, and the generation of heat, and climates, and the celestial bodies.”

But undoubtedly the most remarkable portion of the *Opus Majus* is the Sixth and last Part, which is entitled “De Scientia experimentalis.” It is indeed an extraordinary circumstance to find a writer of the thirteenth century, not only recognizing experiment as one source of knowledge, but urging its claims as something far more important than men had yet been aware of, exemplifying its value by striking and just examples, and speaking of its authority with a dignity of diction which sounds like a foremurmur of the Baconian sentences uttered nearly four hundred years later. Yet this is the character of what we here find†. “Experimental science, the sole mistress of

\* *Op. Maj.*, p. 64.

† “Veritates magnificas in terminis aliarum scientiarum in quas per nullam viam possunt illæ scientia, hæc sola scientiarum domina speculativarum, potest dare.” *Op. Maj.*, p. 465.

speculative sciences, has three great Prerogatives among other parts of knowledge: First she tests by experiment the noblest conclusions of all other sciences: Next she discovers respecting the notions which other sciences deal with, magnificent truths to which these sciences of themselves can by no means attain: her Third dignity is, that she by her own power and without respect of other sciences, investigates the secrets of nature."

The examples which Bacon gives of these "Prerogatives" are very curious, exhibiting, among some error and credulity, sound and clear views. His leading example of the First Prerogative, is the Rainbow, of which the cause, as given by Aristotle, is tested by reference to experiment with a skill which is, even to us now, truly admirable. The examples of the Second Prerogative are three:—*first*, the art of making an artificial sphere which shall move with the heavens by natural influences, which Bacon trusts may be done, though astronomy herself cannot do it—"et tunc," he says, "thesaurum unius regis valeret hoc instrumentum;"—*secondly*, the art of prolonging life, which experiment may teach, though medicine has no means of securing it except by regimen\*;—*thirdly*, the art of making gold finer than fine gold, which goes beyond the power of alchemy. The Third Prerogative of experimental science, arts independent of the received sciences, is exemplified in many curious examples, many of them whimsical tra-

\* One of the ingredients of a preparation here mentioned, is the flesh of a dragon, which, it appears, is used as food by the Ethiopians. The mode of preparing this food cannot fail to amuse the reader. "Where there are good flying dragons, by the art which they possess, they draw them out of their dens, and have bridles and saddles in readiness, and they ride upon them, and make them bound about in the air in a violent manner, that the hardness and toughness of the flesh may be reduced, as boars are hunted and bulls are baited before they are killed for eating." *Op. Maj.*, p. 470.

ditions. Thus it is said that the character of a people may be altered by altering the air\*. Alexander, it seems, applied to Aristotle to know whether he should exterminate certain nations which he had discovered, as being irreclaimably barbarous; to which the philosopher replied, "If you can alter their air, permit them to live, if not, put them to death." In this part, we find the suggestion that the fire-works made by children, of salt-petre, might lead to the invention of a formidable military weapon.

It could not be expected that Roger Bacon, at a time when experimental science hardly existed, could give any *precepts* for the discovery of truth by experiment. But nothing can be a better *example* of the method of such investigation, than his inquiry concerning the cause of the Rainbow. Neither Aristotle, nor Avicenna, nor Seneca, he says, have given us any clear knowledge of this matter, but experimental science can do so. Let the experimenter (*experimentator*) consider the cases in which he finds the same colours, as the hexagonal crystals from Ireland and India; by looking into these he will see colours like these of the rainbow. Many think that this arises from some special virtue of these stones and their hexagonal figure; let therefore the experimenter go on, and he will find the same in other transparent stones, in dark ones as well as in light-coloured. He will find the same effect also in other forms than the hexagon, if they be furrowed in the surface, as the Irish crystals are. Let him consider too, that he sees the same colours in the drops which are dashed from oars in the sunshine;—and in the spray thrown by a mill wheel;—and in the dew drops which lie on the grass in a meadow on a summer morning;—and if a man takes water in his mouth and projects it on one side into a

\* *Op. Maj.*, p. 473.

sunbeam ;—and if in an oil lamp hanging in the air, the rays fall in certain positions upon the surface of the oil ;—and in many other ways, are colours produced. We have here a collection of instances, which are almost all examples of the same kind as the phenomenon under consideration ; and by the help of a principle collected by induction from these facts, the colours of the rainbow were afterwards really explained.

With regard to the form and other circumstances of the bow he is still more precise. He bids us measure the height of the bow and of the sun, to show that the center of the bow is exactly opposite to the sun. He explains the circular form of the bow,—its being independent of the form of the cloud, its moving when we move, its flying when we follow,—by its consisting of the reflections from a vast number of minute drops. He does not, indeed, trace the course of the rays through the drop, or account for the precise magnitude which the bow assumes ; but he approaches to the verge of this part of the explanation ; and must be considered as having given a most happy example of experimental inquiry into nature, at a time when such examples were exceedingly scanty. In this respect, he was more fortunate than Francis Bacon, as we shall hereafter see.

We know but little of the biography of Roger Bacon, but we have every reason to believe that his influence upon his age was not great. He was suspected of magic, and is said to have been put into close confinement in consequence of this charge. In his work he speaks of Astrology, as a science well worth cultivating. “But,” says he, “Theologians and Decretists, not being learned in such matters, and seeing that evil as well as good may be done, neglect and abhor such things, and reckon them among Magic Arts.” We have already seen, that at the very time when Bacon was thus raising his voice against

the habit of blindly following authority, and seeking for all science in Aristotle, Thomas Aquinas was employed in fashioning Aristotle's tenets into that fixed form in which they became the great impediment to the progress of knowledge. It would seem, indeed, that something of a struggle between the progressive and stationary powers of the human mind was going on at this time. Bacon himself says\*, "Never was there so great an appearance of wisdom, nor so much exercise of study in so many Faculties, in so many regions, as for this last forty years. Doctors are dispersed everywhere, in every castle, in every burgh, and especially by the students of two Orders, (he means the Franciscans and Dominicans, who were almost the only religious orders that distinguished themselves by an application to study†,) which has not happened except for about forty years. And yet there was never so much ignorance, so much error." And in the part of his work which refers to Mathematics, he says of that study‡, that it is the door and the key of the sciences; and that the neglect of it for thirty or forty years has entirely ruined the studies of the Latins. According to these statements, some change, disastrous to the fortunes of science, must have taken place about 1230, soon after the foundation of the Dominican and Franciscan Orders§. Nor can we doubt that the adoption of the Aristotelian philosophy by these two Orders, in the form in which the Angelical Doctor had systematized it, was one of the events which most tended to defer, for three centuries, the reform which Roger Bacon urged as a matter of crying necessity in his own time.

\* Quoted by Jebb, *Pref. to Op. Maj.*

† Mosheim, *Hist.* iii. 161.

‡ *Op. Maj.*, p. 57.

§ Mosheim, iii. 161.



## CHAPTER VIII.

## THE REVIVAL OF PLATONISM.

1. *Causes of Delay in the Advance of Knowledge.*—In the insight possessed by learned men into the method by which truth was to be discovered, the fourteenth and fifteenth centuries went backwards, rather than forwards, from the point which had been reached in the thirteenth. Roger Bacon had urged them to have recourse to experiment; but they returned with additional and exclusive zeal to the more favourite employment of reasoning upon their own conceptions. He had called upon them to look at the world without; but their eyes forthwith turned back upon the world within. In the constant oscillation of the human mind between Ideas and Facts, after having for a moment touched the latter, it seemed to swing back more impetuously to the former. Not only was the philosophy of Aristotle firmly established for a considerable period, but when men began to question its authority, they attempted to set up in its place a philosophy still more purely ideal, that of Plato. It was not till the actual progress of experimental knowledge for some centuries had given it a vast accumulation of force, that it was able to break its way fully into the circle of speculative science. The new Platonist schoolmen had to run their course, the practical discoverers had to prove their merit by their works, the Italian innovators had to utter their aspirations for a change, before the second Bacon could truly declare that the time for a fundamental reform was at length arrived.

It cannot but seem strange, to any one who attempts to trace the general outline of the intellectual progress of man, and who considers him as under the guidance of

a Providential sway, that he should thus be permitted to wander so long in a wilderness of intellectual darkness; and even to turn back, by a perverse caprice as it might seem, when on the very border of the brighter and better land which was his destined inheritance. We do not attempt to solve this difficulty: but such a course of things naturally suggests the thought, that a progress in physical science is not the main object of man's career, in the eyes of the Power who directs the fortunes of our race. We can easily conceive that it may have been necessary to man's general welfare that he should continue to turn his eyes inwards upon his own heart and faculties, till Law and Duty, Religion and Government, Faith and Hope, had been fully incorporated with all the past acquisitions of human intellect; rather than that he should have rushed on into a train of discoveries tending to chain him to the objects and operations of the material world. The systematic Law\* and philosophical Theology which acquired their ascendancy in men's minds at the time of which we speak, kept them engaged in a region of speculations which perhaps prepared the way for a profounder and wider civilization, for a more elevated and spiritual character, than might have been possible without such a preparation. The great Italian poet of the fourteenth century speaks with strong admiration of the founders of the system which prevailed in his time. Thomas, Albert, Gratian, Peter Lombard, occupy distinguished places in the Paradise. The first, who is the poet's instructor, says,—

Io fui degli agni della santa greggia  
 Che Domenico mena per cammino  
 U' ben s'impingua se non si vaneggia.  
 Questo che m'è a destra piu vicino

\* Gratian published the *Decretals* in the twelfth century; and the Canon and Civil Law became a regular study in the universities soon afterwards.

Frate e maestro fummi; ed esso Alberto  
 E di Cologne, ed io Tomas d'Aquino. . . . .  
 Quell' altro fiammeggiar esce del riso  
 De Grazian, che l'uno et l'altro foro  
 Ajutò sì che piace in Paradiso.

I, then, was of the lambs that Dominic  
 Leads, for his saintly flock, along the way  
 Where well they thrive not swoln with vanity.  
 He nearest on my right-hand brother was  
 And master to me; Albert of Cologne  
 Is this; and of Aquinũ Thomas, I. . . . .  
 That next resplendence issues from the smile  
 Of Gratian who to either forum lent  
 Such help as favour wins in Paradise.

It appears probable that neither poetry, nor painting, nor the other arts which require for their perfection a lofty and spiritualized imagination, would have appeared in the noble and beautiful forms which they assumed in the fourteenth and fifteenth century, if men of genius had, at the beginning of that period, made it their main business to discover the laws of nature, and to reduce them to a rigorous scientific form. Yet who can doubt that the absence of these touching and impressive works would have left one of the best and purest parts of man's nature without its due nutriment and developement? It may perhaps be a necessary condition in the progress of man, that the Arts which aim at beauty should reach their excellence before the Sciences which seek speculative truth; and if this be so, we inherit, from the middle ages, treasures which may well reconcile us to the delay which took place in their cultivation of experimental science.

However this may be, it is our business at present to trace the circumstances of this very lingering advance. We have already noticed the contest of the Nominalists and Realists, which was one form, though, with regard

to scientific methods, an unprofitable one, of the anti-thesis of Ideas and Things. Though, therefore, this struggle continued, we need not dwell upon it. The Nominalists denied the real existence of Ideas, which doctrine was to a great extent implied in the prevailing systems; but the controversy in which they thus engaged, did not lead them to seek for knowledge in a new field and by new methods. The arguments which Occam the Nominalist opposes to those of Duns Scotus the Realist, are marked with the stamp of the same system, and consist only in permutations and combinations of the same elementary conceptions. It was not till the impulse of external circumstances was added to the discontent, which the more stirring intellects felt towards the barren dogmatism of their age, that the activity of the human mind was again called into full play, and a new career of progression entered upon, till then undreamt of, except by a few prophetic spirits.

2. *Causes of Progress.*—These circumstances were principally the revival of Greek and Roman literature, the invention of Printing, the Protestant Reformation, and a great number of curious discoveries and inventions in the arts, which were soon succeeded by important steps in speculative physical science. Connected with the first of these events, was the rise of a party of learned men who expressed their dissatisfaction with the Aristotelian philosophy, as it was then taught, and manifested a strong preference for the views of Plato. It is by no means suitable to our plan to give a detailed account of this new Platonic school; but we may notice a few of the writers who belong to it, so far at least as to indicate its influence upon the Methods of pursuing science.

In the fourteenth century\*, the frequent intercourse of the most cultivated persons of the Eastern and West-

\* Tenneman, ix. 14.

ern Empire, the increased study of the Greek language in Italy, the intellectual activity of the Italian States, the discovery of manuscripts of the classical authors, were circumstances which excited or nourished a new and zealous study of the works of Greek and Roman genius. The genuine writings of the ancients, when presented in their native life and beauty, instead of being seen only in those lifeless fragments and dull transformations which the scholastic system had exhibited, excited an intense enthusiasm. Europe, at that period, might be represented by Plato's beautiful allegory, of a man who, after being long kept in a dark cavern, in which his knowledge of the external world is gathered from the images which stream through the chinks of his prison, is at last led forth into the full blaze of day. It was inevitable that such a change should animate men's efforts and enlarge their faculties. Greek literature became more and more known, especially by the influence of learned men who came from Constantinople into Italy: these teachers, though they honoured Aristotle, revered Plato no less, and had never been accustomed to follow with servile submission of thought either these or any other leaders. The effect of such influences soon reveals itself in the works of that period. Dante has woven into his *Divina Comedia* some of the ideas of Platonism. Petrarch, who had formed his mind by the study of Cicero, and had thus been inspired with a profound admiration for the literature of Greece, learnt Greek from Barlaam, a monk who came as ambassador from the Emperor of the East to the Pope, in 1339. With this instructor, the poet read the works of Plato; struck by their beauty, he contributed, by his writings and his conversation, to awake in others an admiration and love for that philosopher, which soon became strongly and extensively prevalent among the learned in Italy.

3. *Hermolaus Barbarus, &c.*—Along with this feeling there prevailed also, among those who had learnt to relish the genuine beauties of the Greek and Latin writers, a strong disgust for the barbarisms in which the scholastic philosophy was clothed. Hermolaus Barbarus\*, who was born in 1454, at Venice, and had formed his taste by the study of classical literature, translated, among other learned works, Themistius's paraphrastic exposition of the Physics of Aristotle; with the view of trying whether the Aristotelian Natural Philosophy could not be presented in good Latin, which the scholastic teachers denied. In his Preface he expresses great indignation against those philosophers who have written and disputed on philosophical subjects in barbarous Latin, and in an uncultured style, so that all refined minds are repelled from these studies by weariness and disgust. They have, he says, by this barbarism, endeavoured to secure to themselves, in their own province, a supremacy without rivals or opponents. Hence they maintain that mathematics, philosophy, jurisprudence, cannot be expounded in correct Latin;—that between these sciences and the genuine Latin language there is a great gulf, as between things that cannot be brought together: and on this ground they blame those who combine the study of philology and eloquence with that of science. This opinion, adds Hermolaus, perverts and ruins our studies; and is highly prejudicial and unworthy in respect to the state. Hermolaus awoke in others, as for instance, in John Picus of Mirandula, the same dislike to the reigning school philosophy. As an opponent of the same kind, we may add Marius Nizolius of Bersallo, a scholar who carried his admiration of Cicero to an exaggerated extent, and who was led, by a controversy with the defenders of the scholastic philosophy, to pub-

\* Tenneman, ix. 25.

lish (1553) a work *On the True Principles and True Method of Philosophizing*. In the title of this work, he professes to give "the true principles of almost all arts and sciences, refuting and rejecting almost all the false principles of the Logicians and Metaphysicians." But although, in the work, he attacks the scholastic philosophy, he does little or nothing to justify the large pretensions of his title; and he excited, it is said, little notice. It is therefore curious that Leibnitz should have thought it worth his while to re-edit this work, which he did in 1670, adding remarks of his own.

4. *Nicolaus Cusanus*.—Without dwelling upon this opposition to the scholastic system on the ground of taste, I shall notice somewhat further those writers who put forwards Platonic views, as fitted to complete or to replace the doctrines of Aristotle. Among these, I may place Nicolaus Cusanus, so called from Cus, a village on the Moselle, where he was born in 1401; who was afterwards raised to the dignity of cardinal. We might, indeed, at first be tempted to include Cusanus among those persons who were led to reject the old philosophy by being themselves agents in the progressive movement of physical science. For he published, before Copernicus, and independently of him, the doctrine that the earth is in motion\*. But it should be recollected that in order to see the possibility of this doctrine, and its claims to acceptance, no new reference to observation was requisite. The Heliocentric System was merely a new mode of representing to the mind facts with which all astronomers had long been familiar. The system might very easily have been embraced and inculcated by Plato himself; as indeed it is said to have been actually taught by Pythagoras. The mere adoption of the Heliocentric view,

\* "Jam nobis manifestum est terram istam in veritate moveri," &c.  
—*De Doctâ Ignorantiâ*, Lib. II. cap. 12.

therefore, without attempting to realize the system in detail, as Copernicus did, cannot entitle a writer of the fifteenth century to be looked upon as one of the authors of the discoveries of that period; and we must consider Cusanus as a speculative anti-Aristotelian, rather than as a practical reformer.

The title of Cusanus's book, *De Doctâ Ignorantiâ*, shows how far he was from agreeing with those who conceived that, in the works of Aristotle, they had a full and complete system of all human knowledge. At the outset of this book\*, he says, after pointing out some difficulties in the received philosophy, "If, therefore, the case be so, (as even the most profound Aristotle, in his *First Philosophy*, affirms,) that in things most manifest by nature, there is a difficulty, no less than for an owl to look at the sun; since the appetite of knowledge is not implanted in us in vain, we ought to desire to know that we are ignorant. If we can fully attain to this, we shall arrive at *Instructed Ignorance*." How far he was from placing the source of knowledge in experience, as opposed to ideas, we may see in the following passage† from another work of his, *On Conjectures*. "Conjectures must proceed from our mind, as the real world proceeds from the infinite Divine Reason. For since the human mind, the lofty likeness of God, participates, as it may, in the fruitfulness of the creative nature, it doth from itself, as the image of the Omnipotent Form, bring forth reasonable thoughts which have a similitude to real existences. Thus the Human Mind exists as a conjectural form of the world, as the Divine Mind is its real form." We have here the Platonic or ideal side of knowledge put prominently and exclusively forwards.

5. *Marsilius Ficinus*, &c.—A person who had much more influence on the diffusion of Platonism was Marsi-

\* *De Doct. Ignor.*, Lib. 1. c. 1.      † *De Conjecturis*, Lib. 1. c. 3, 4.



lius Ficinus, a physician of Florence. In that city there prevailed, at the time of which we speak, the greatest enthusiasm for Plato. George Gemistius Pletho, when in attendance upon the Council of Florence, had imparted to many persons the doctrines of the Greek philosopher; and, among others, had infused a lively interest on this subject into the elder Cosmo, the head of the family of the Medici. Cosmo formed the plan of founding a Platonic academy. Ficinus\*, well instructed in the works of Plato, Plotinus, Proclus, and other Platonists, was selected to further this object, and was employed in translating the works of these authors into Latin. It is not to our present purpose to consider the doctrines of this school, except so far as they bear upon the nature and methods of knowledge; and therefore I must pass by, as I have in other instances done, the greater part of their speculations, which related to the nature of God, the immortality of the soul, the principles of Goodness and Beauty, and other points of the same order. The object of these and other Platonists of this school, however, was not to expel the authority of Aristotle by that of Plato. Many of them had come to the conviction that the highest ends of philosophy were to be reached only by bringing into accordance the doctrines of Plato and of Aristotle. Of this opinion was John Picus, Count of Mirandula and Concordia; and under this persuasion he employed the whole of his life in labouring upon a work, *De Concordiâ Platonis et Aristotelis*, which was not completed at the time of his death, in 1494; and has never been published. But about a century later, another writer of the same school, Francis Patricius†, pointing out the discrepancies between the two Greek teachers, urged the propriety of deposing Aristotle from the supremacy he had so long enjoyed. "Now all these

\* Born in 1433.

† Born 1529, died 1597.

doctrines, and others not a few," he says\*, "since they are Platonic doctrines, philosophically most true, and consonant with the Catholic faith, whilst the Aristotelian tenets are contrary to the faith, and philosophically false, who will not, both as a Christian and a philosopher, prefer Plato to Aristotle? And why should not hereafter, in all the colleges and monasteries of Europe, the reading and study of Plato be introduced? Why should not the philosophy of Aristotle be forthwith exiled from such places? Why must men continue to drink the mortal poison of impiety from that source?" with much more in the same strain.

The Platonic school, of which we have spoken, had, however, reached its highest point of prosperity before this time, and was already declining. About 1500, the Platonists appeared to triumph over the Peripatetics†; but the death of their great patron, Cardinal Bessarion, about this time, and we may add, the hollowness of their system in many points, and its want of fitness for the wants and expectations of the age, turned men's thoughts partly back to the established Aristotelian doctrines, and partly forwards to schemes of bolder and fresher promise.

6. *Francis Patricius*.—Patricius, of whom we have just spoken, was one of those who had arrived at the conviction that the formation of a new philosophy, and not merely the restoration of an old one, was needed. In 1593, appeared his *Nova de Universis Philosophia*; and the mode in which it begins‡ can hardly fail to remind us of the expressions which Francis Bacon soon afterwards used in the opening of a work of the same nature. "Francis Patricius, being about to found anew the true

\* *Aristoteles Exotericus*, p. 50.

† Tiraboschi, t. vii. part ii. p. 411.

‡ "Franciscus Patricius, novam veram integram de universis conditurus philosophiam, sequentia uti verissima prænuntiare est ausus.  
Præ-

philosophy of the universe, dared to begin by announcing the following indisputable principles." Here, however, the resemblance between Patricius and true inductive philosophers ends. His principles are barren *à priori* axioms; and his system has one main element, *Light*, (*Lux*, or *Lumen*,) to which all operations of nature are referred. In general cultivation, and practical knowledge of nature, he was distinguished among his contemporaries. In various passages of his works he relates\* observations which he had made in the course of his travels, in Cyprus, Corfu, Spain, the mountains of the Modenese, and Dalmatia, which was his own country; his observations relate to light, the saltiness of the sea, its flux and reflux, and other points of astronomy, meteorology, and natural history. He speaks of the sex of plants†; rejects judicial astrology; and notices the astronomical systems of Copernicus, Tycho, Fracastoro, and Torre. But the mode in which he speaks of experiments proves, what indeed is evident from the general scheme of his system, that he had no due appreciation of the place which observation must hold in real and natural philosophy.

7. *Picus, Agrippa, &c.*—It had been seen in the later philosophical history of Greece, how readily the ideas of the Platonic school lead on to a system of unfathomable and unbounded mysticism. John Picus, of Mirandula‡, added to the study of Plato and the Neoplatonists, a mass of allegorical interpretations of the Scriptures, and the

Prænunciata ordine persecutus, divinis oraculis, geometricis rationibus, clarissimisque experimentis comprobavit.

Ante primum nihil,

Post primum omnia,

A principio omnia," &c.

His other works are *Panaugia*, *Pancosmia*, *Dissertationes Peripateticæ*.

\* Tiraboschi, t. vii. part ii. p. 411.

† *Dissert. Peripatet.*, t. ii. lib. v. sub fin.

‡ Tenneman, ix. 148.

dreams of the Cabbala, a Jewish system \*, which pretends to explain how all things are an emanation of the Deity. To this his nephew, Francis Picus, added a reference to inward illumination †, by which knowledge is obtained, independently of the progress of reasoning. John Reuchlin, or Capnio, born 1455; John Baptist Helmont, born 1577; Francis Mercurius Helmont, born 1618, and others, succeeded John Picus in his admiration of the Cabbala: while others, as Jacob Boehmen, rested upon internal revelations like Francis Picus. And thus we have a series of mystical writers, continued into modern times, who may be considered as the successors of the Platonic school; and who all exhibit views altogether erroneous with regard to the nature and origin of knowledge. Among the various dreams of this school are certain wide and loose analogies of terrestrial and spiritual things. Thus in the writings of Cornelius Agrippa (who was born 1487, at Cologne) we have such systems as the following ‡: —“Since there is a threefold world, elemental, celestial, and intellectual, and each lower one is governed by that above it, and receives the influence of its powers: so that the very Archetype and Supreme Author transfuses the virtues of his omnipotence into us through angels, heavens, stars, elements, animals, plants, stones,—into us, I say, for whose service he has framed and created all these things;—the Magi do not think it irrational that we should be able to ascend by the same degrees, the same worlds, to this Archetype of the world, the Author and First Cause of all, of whom all things are, and from whom they proceed; and should not only avail ourselves of those powers which exist in the nobler works of creation, but also should be able to attract other powers, and add them to these.”

\* Tenneman, ix. 167.

† *Ib.*, 158.

‡ Agrippa, *De Occult. Phil.*, Lib. i. c. l.

Agrippa's work, *De Vanitate Scientiarum*, may be said rather to have a skeptical and cynical, than a Platonic, character. It is a declamation\*, in a melancholy mood, against the condition of the sciences in his time. His indignation at the worldly success of men whom he considered inferior to himself, had, he says, metamorphosed him into a dog, as the poets relate of Hecuba of Troy, so that his impulse was to snarl and bark. His professed purpose, however, was to expose the dogmatism, the servility, the self-conceit, and the neglect of religious truth which prevailed in the reigning Schools of philosophy. His views of the nature of science, and the modes of improving its cultivation, are too imperfect and vague to allow us to rank him among the reformers of science.

8. *Paracelsus, Fludd, &c.*—The celebrated Paracelsus† put himself forwards as a reformer in philosophy, and obtained no small number of adherents. He was, in most respects, a shallow and impudent pretender; and had small knowledge of the literature or science of his time: but by the tone of his speaking and writing he manifestly belongs to the mystical school of which we are now speaking. Perhaps by the boldness with which he proposed new systems, and by connecting these with the practical doctrines of medicine, he contributed something to the introduction of a new philosophy. We have seen in the History of Chemistry that he was the author of the system of Three Principles, (salt, sulphur, and mercury,) which replaced the ancient doctrine of Four Elements, and prepared the way for a true science of chemistry. But the salt, sulphur, and mercury of Paracelsus were not, he tells his disciples, the visible bodies which

\* Written in 1526.

† Philip Aurelius Theophrastus Bombastus von Hohenheim, also called Paracelsus Eremita, born at Einsiedlen in Switzerland, in 1493.

we call by those names, but certain invisible, astral, or sidereal elements. The astral salt is the basis of the solidity and incombustible parts in bodies; the astral sulphur is the source of combustion and vegetation; the astral mercury is the origin of fluidity and volatility. And again, these three elements are analogous to the three elements of man,—Body, Spirit, and Soul.

A writer of our own country, belonging to this mystical school, is Robert Fludd, or De Fluctibus, who was born in 1571, in Kent, and after pursuing his studies at Oxford, travelled for several years. Of all the Theosophists and Mystics, he is by much the most learned; and was engaged in various controversies with Mersenne, Gassendi, Kepler, and others. He thus brings us in contact with the next class of philosophers whom we have to consider, the practical reformers of philosophy;—those who furthered the cause of science by making, promulgating, or defending the great discoveries which now began to occupy men. He adopted the principle, which we have noticed elsewhere\*, of the analogy of the Macrocosm and Microcosm, the world of nature and the world of man. His system contains such a mixture and confusion of physical and metaphysical doctrines as might be expected from his ground-plan, and from his school. Indeed his object, the general object of mystical speculators, is to identify physical with spiritual truths. Yet the influence of the practical experimental philosophy which was now gaining ground in the world may be traced in him. Thus he refers to experiments on distillation to prove the existence and relation of the regions of water, air, and fire, and of the spirits which correspond to them; and is conceived, by some persons†, to have anticipated Torricelli in the invention of the Barometer.

\* B. ix. c. 2. s. 1. The Mystical School of Biology.

† Tenneman, ix. 221.

We need no further follow the speculations of this school. We see already abundant reason why the reform of the methods of pursuing science could not proceed from the Platonists. Instead of seeking knowledge by experiment, they immersed themselves deeper than even the Aristotelians had done in traditionary lore, or turned their eyes inwards in search of an internal illumination. Some attempts were made to remedy the defects of philosophy by a recourse to the doctrines of other sects of antiquity, when men began to feel more distinctly the need of a more connected and solid knowledge of nature than the established system gave them. Among these attempts were those of Berigard\*, Magernus, and especially Gassendi, to bring into repute the philosophy of the Ionian school, of Democritus and of Epicurus. But these endeavours were posterior in time to the new impulse given to knowledge by Copernicus, Kepler, and Galileo, and were influenced by views arising out of the success of these discoveries, and they must, therefore, be considered hereafter. In the mean time, some independent efforts (arising from speculative rather than practical reformers) were made to cast off the yoke of the Aristotelian dogmatism, and to apprehend the true form of that new philosophy which the most active and hopeful minds saw to be needed; and we must give some account of these attempts, before we can commit ourselves to the full stream of progressive philosophy.

\* Tenneman, 265.

## CHAPTER IX.

## THE THEORETICAL REFORMERS OF SCIENCE.

WE have already seen that Patricius, about the middle of the sixteenth century, announced his purpose of founding anew the whole fabric of philosophy; but that, in executing this plan, he ran into wide and baseless hypotheses, suggested by *à priori* conceptions rather than by external observation; and that he was further misled by fanciful analogies resembling those which the Platonic mystics loved to contemplate. The same time, and the period which followed it, produced several other essays which were of the same nature, with the exception of their being free from the peculiar tendencies of the Platonic school: and these insurrections against the authority of the established dogmas, although they did not directly substitute a better positive system in the place of that which they assailed, shook the authority of the Aristotelian system, and led to its overthrow; which took place as soon as these theoretical were aided by other practical reformers.

*Bernardinus Telesius.*—Italy, always, in modern times, fertile in the beginnings of new systems, was the soil on which these innovators arose. These earliest and most conspicuous of them is Bernardinus Telesius, who was born in 1508, at Cosenza, in the kingdom of Naples. His studies, carried on with great zeal and ability, first at Milan and then at Rome, made him well acquainted with the knowledge of his times; but his own reflections convinced him that the basis of science, as then received, was altogether erroneous; and led him to attempt a reform, with which view, in 1565, he published, at Rome, his work\*, "*Bernardinus Telesius, of Cosenza, on the*

\* Bernardini Telesii Consentini *De Rerum Natura juxta propria Principia.*



*Nature of Things, according to principles of his own."*

In the preface of this work he gives a short account\* of the train of reflection by which he was led to put himself in opposition to the Aristotelian philosophy. This kind of autobiography occurs not unfrequently in the writings of theoretical reformers; and shows how lively they felt the novelty of their undertaking. After the storm and sack of Rome in 1527, Telesius retired to Padua, as a peaceful seat of the muses; and there studied philosophy and mathematics, with great zeal, under the direction of Jerom Amalthæus and Frederic Delphinus. In these studies he made great progress; and the knowledge which he thus acquired threw a new light upon his view of the Aristotelian philosophy. He undertook a closer examination of the Physical Doctrines of Aristotle; and as the result of this, he was astonished how it could have been possible that so many excellent men, so many nations, and even almost the whole human race, should, for so long a time, have allowed themselves to be carried away by a blind reverence for a teacher, who had committed errors so numerous and grave as he perceived to exist in "the philosopher." Along with this view of the insufficiency of the Aristotelian philosophy, arose, at an early period, the thought of erecting a better system in its place. With this purpose he left Padua, when he had received the degree of Doctor, and went to Rome, where he was encouraged in his design by the approval and friendly exhortations of distinguished men of letters, amongst whom were Ubaldino Bandinelli and Giovanni della Casa. From Rome he went to his native place, when the incidents and occupations of a married life for a while interrupted his phi-

\* I take this account from Tenneman: this Proem was omitted in subsequent editions of Telesius, and is not in the one which I have consulted. Tenneman, *Gesch. d. Phil.*, ix. 280.

losophical project. But after his wife was dead, and his eldest son grown to manhood, he resumed with ardour the scheme of his youth; again studied the works of Aristotle and other philosophers, and composed and published the first two books of his treatise. The opening to this work sufficiently exhibits the spirit in which it was conceived. Its object is stated in the title to be to show, that "the construction of the world, the magnitude and nature of the bodies contained in it, are not to be investigated by reasoning, which was done by the ancients, but are to be apprehended by the senses, and collected from the things themselves." And the Proem is in the same strain. "They who before us have inquired concerning the construction of this world and of the things which it contains, seem indeed to have prosecuted their examination with protracted vigils and great labour, but *never to have looked at it.*" And thus, he observes, they found nothing but error. This he ascribes to their presumption. "For, as it were, attempting to rival God in wisdom, and venturing to seek for the principles and causes of the world by the light of their own reason, and thinking they had found what they had only invented, they made an arbitrary world of their own." "*We* then," he adds, "not relying on ourselves, and of a duller intellect than they, propose to ourselves to turn our regards to the world itself and its parts."

The execution of the work, however, by no means corresponds to the announcement. The doctrines of Aristotle are indeed attacked; and the objections to these, and to other received opinions, form a large part of the work. But these objections are supported by *a priori* reasoning, and not by experiments. And thus, rejecting the Aristotelian physics, he proposes a system at least equally baseless; although, no doubt, grateful

to the author from its sweeping and apparently simple character. He assumes three principles, Heat, Cold, and Matter: Heat is the principle of motion, Cold of immobility, and Matter is the corporeal substratum, in which these incorporeal and active principles produce their effects. It is easy to imagine that, by combining and separating these abstractions in various ways, a sort of account of many natural phenomena may be given; but it is impossible to ascribe any real value to such a system. The merit of Telesius must be considered to consist in his rejection of the Aristotelian errors, in his perception of the necessity of a reform in the method of philosophizing, and in his persuasion that this reform must be founded on experiments rather than on reasoning. When he said\*, "We propose to ourselves to turn our eyes to the world itself, and its parts, their passions, actions, operations and species," his view of the course to be followed was right; but his purpose remained but ill fulfilled, by the arbitrary edifice of abstract conceptions which his system exhibits.

Francis Bacon, who, about half a century later, treated the subject of a reform of philosophy in a far more penetrating and masterly manner, has given us his judgment of Telesius. In his view, he considers Telesius as the restorer of the Atomic philosophy, which Democritus and Parmenides taught among the ancients; and according to his custom, he presents an image of this philosophy in an adaptation of a portion of ancient mythology†. The Celestial Cupid, who, with Cœlus, was the parent of the Gods and of the Universe, is exhibited as a representation of matter and its properties, according to the

\* Proem.

† "De Principiis atque Originibus secundum fabulas Cupidinis et Cœli: sive Parmenidis et Telesii et præcipuè Democriti Philosophia tractata in Fabula de Cupidine."

Democritean philosophy. "Concerning Telesius," says Bacon, "we think well, and acknowledge him as a lover of truth, a useful contributor to science, an amender of some tenets, the first of recent men. But we have to do with him as the restorer of the philosophy of Parmenides, to whom much reverence is due." With regard to this philosophy, he pronounces a judgment which very truly expresses the cause of its rashness and emptiness. "It is," he says, "such a system\* as naturally proceeds from the intellect, abandoned to its own impulse, and not rising from experience to theory continuously and successively." Accordingly, he says that, "Telesius, although learned in the Peripatetic philosophy (if that were anything), which indeed, he has turned against the teachers of it, is hindered by his affirmations, and is more successful in destroying than in building."

The work of Telesius excited no small notice, and was placed in the *Index Expurgatorius*. It made many disciples, a consequence probably due to its spirit of system-making, no less than to its promise of reform, or its acuteness of argument; for till trial and reflection have taught man modesty and moderation, he can never be content to receive knowledge in the small successive instalments in which nature gives it forth to him. It is the makers of large systems, arranged with an appearance of completeness and symmetry, who, principally, give rise to Schools of philosophy.

(*Thomas Campanella*).—Accordingly, Telesius may be looked upon as the founder of a School. His most distinguished successor was Thomas Campanella, who was born in 1568, at Stilo, in Calabria. He showed great talents at an early age, prosecuting his studies at Cosenza,

\* "Talia sunt qualia possunt esse ea quæ ab intellectu sibi permisso, nec ab experimentis continenter et gradatim sublevato, profecta videntur."

the birth-place of the great opponent of Aristotle and reformer of philosophy. He, too, has given us an account\* of the course of thought by which he was led to become an innovator. "Being afraid that not genuine truth, but falsehood in the place of truth, was the tenant of the Peripatetic School, I examined all the Greek, Latin, and Arabic commentators of Aristotle, and hesitated more and more, as I sought to learn whether what they have said were also to be read in the world itself, which I had been taught by learned men was the living book of God. And as my doctors could not satisfy my scruples, I resolved to read all the books of Plato, Pliny, Galen, the Stoics, and the Democriteans, and especially those of Telesius; and to compare them with that *first and original writing, the world*; that thus from the primary autograph, I might learn if the copies contained anything false." Campanella probably refers here to an expression of Plato, who says, "the world is God's epistle to mankind." And this image, of the natural world as an original manuscript, while human systems of philosophy are but copies, and may be false ones, became a favourite thought of the reformers, and appears repeatedly in their writings from this time. "When I held my public disputation at Cosenza," Campanella proceeds, "and still more, when I conversed privately with the brethren of the monastery, I found little satisfaction in their answers; but Telesius delighted me, on account of his freedom in philosophizing, and because he rested upon the nature of things, and not upon the assertions of men."

With these views and feelings, it is not wonderful that Campanella, at the early age of twenty-two (1590,) published a work remarkable for the bold promise of its

\* Thom. Campanella *de Libris propriis*, as quoted in Tenneman, ix. 291.

title: "*Thomas Campanella's Philosophy demonstrated to the senses, against those who have philosophized in a arbitrary and dogmatical manner, not taking nature for their guide; in which the errours of Aristotle and his followers are refuted from their own assertions and the laws of nature; and all the imaginations feigned in the place of nature by the Peripatetics are altogether rejected; with a true defence of Bernardin Telesius of Cosenza, the greatest of philosophers; confirmed by the opinions of the ancients, here elucidated and defended, especially those of the Platonists.*"

This work was written in answer to a book published against Telesius by a Neapolitan professor named Marta; and it was the boast of the young author that he had only employed eleven months in the composition of his defence, while his adversary had been engaged eleven years in preparing his attack. Campanella found a favourable reception in the house of the Marchese Lavelli, and there employed himself in the composition of an additional work, entitled *On the Sense of Things and Magic*, and in other literary labours. These, however, are full of the indications of an enthusiastic temper, inclined to mystical devotion, and of opinions bearing the cast of pantheism. For instance, the title of the book last quoted sets forth as demonstrated in the course of the work, that "the world is the living and intelligent statue of God; and that all its parts, and particles of parts, are endowed some with a clearer, some with a more obscure sense, such as suffices for the preservation of each and of the whole." Besides these opinions, which could not fail to make him obnoxious to the religious authorities, Campanella\* engaged in schemes of political revolution, which involved him in danger and calamity. He took part in a conspiracy, of which the

\* *Economisti Italiani*, Tom. i. p. xxxiii.

object was to cast off the tyranny of Spain, and to make Calabria a republic. This design was discovered; and Campanella, along with others, was thrown into prison and subjected to torture. He was kept in confinement twenty-seven years; and at last obtained his liberation by the interposition of Pope Urban VIII. He was, however, still in danger from the Neapolitan Inquisition; and escaped in disguise to Paris, where he received a pension from the king, and lived in intercourse with the most eminent men of letters. He died there in 1639.

Campanella was a contemporary of Francis Bacon, whom we must consider as belonging to an epoch to which the Calabrian school of innovators was only a prelude. I shall not therefore further follow the connexion of writers of this order. Tobias Adami, a Saxon writer, an admirer of Campanella's works, employed himself, about 1620, in adapting them to the German public, and in recommending them strongly to German philosophers. Descartes, and even Bacon, may be considered as successors of Campanella; for they too were theoretical reformers; but they enjoyed the advantage of the light which had, in the mean time, been thrown upon the philosophy of science, by the great practical advances of Kepler, Galileo, and others. To these practical reformers we must soon turn our attention; but we may first notice one or two additional circumstances belonging to our present subject.

Campanella remarks that both the Peripatetics and the Platonists conducted the learner to knowledge by a long and circuitous path, which he wished to shorten by setting out from the sense. Without speaking of the methods which he proposed, we may notice one maxim\* of considerable value which he propounds, and to which we have already been led. "We begin to reason from

\* Tenneman, ix. 305.

sensible objects, and definition is the end and epilogue of science. It is not the beginning of our knowing, but only of our teaching."

(*Andrew Cæsalpinus.*)—The same maxim had already been announced by Cæsalpinus, a contemporary of Telesius; (he was born at Arezzo in 1520, and died at Rome in 1603.) Cæsalpinus is a great name in science, though professedly an Aristotelian. It has been seen in the *History of Science*\*, that he formed the first great epoch of the science of botany by his systematic arrangement of plants, and that in this task he had no successor for nearly a century. He also approached near to the great discovery of the circulation of the blood†. He takes a view of science which includes the remark that we have just quoted from Campanella: "We reach perfect knowledge by three steps: Induction, Division, Definition. By Induction, we collect likeness and agreement from observation; by Division, we collect unlikeness and disagreement; by Definition, we learn the proper substance of each object. Induction makes universals from particulars, and offers to the mind all intelligible matter; Division discovers the difference of universals, and leads to species; Definition resolves species into their principles and elements‡." Without asserting this to be rigorously correct, it is incomparably more true and philosophical than the opposite view, which represents definition as the beginning of our knowledge; and the establishment of such a doctrine is a material step in inductive philosophy§.

(*Giordano Bruno.*)—Among the Italian innovators of this time we must notice the unfortunate Giordano Bruno, who was born at Nola about 1550, and burnt at Rome in 1600. He is, however, a reformer of a different

\* *Hist. Ind. Sci.*, B. xvi. c. iii. sect. 2.

† *Ib.*, B. xvii. ch. ii. sect. 1.

‡ *Quæst. Peripateticæ*, 1. 1.

§ Tenneman, ix. 108.



school from Campanella; for he derives his philosophy from Ideas and not from Observation. He represents himself as the author of a new doctrine, which he terms the *Nolan Philosophy*. He was a zealous promulgator and defender of the Copernican system of the universe, as we have noticed in the *History of Science*\*. Campanella also wrote in defence of that system.

It is worthy of remark that a thought which is often quoted from Francis Bacon, occurs in Bruno's *Cena di Cenere*, published in 1584; I mean, the notion that the later times are more aged than the earlier. In the course of the dialogue, the Pedant, who is one of the interlocutors, says, "In antiquity is wisdom;" to which the Philosophical Character replies, "If you knew what you were talking about, you would see that your principle leads to the opposite result of that which you wish to infer;—I mean, that *we* are older, and have lived longer, than our predecessors." He then proceeds to apply this, by tracing the course of astronomy through the earlier astronomers up to Copernicus.

(*Peter Ramus*.)—I will notice one other reformer of this period, who attacked the Aristotelian system on another side, on which it was considered to be most impregnable. This was Peter Ramus, (born in Picardy in 1515,) who ventured to denounce the *Logic* of Aristotle as unphilosophical and useless. After showing an extraordinary aptitude for the acquirement of knowledge in his youth, when he proceeded to the degree of Master of Arts, he astonished his examiners by choosing for the subject of the requisite disputation the thesis†, "that all which Aristotle has said is not true." This position, so startling in 1535, he defended for the whole day, without being defeated. This was, however, only a formal academical exercise, which did not necessarily imply any per-

\* *Hist. Ind. Sci.*, B. v. c. iii. sect. 2.    † Tenneman, ix. 420.

manent conviction of the opinion thus expressed. But his mind was really labouring to detect and remedy the errors which he thus proclaimed. From him, as from the other reformers of this time, we have an account of this mental struggle\*. He says, in a work on this subject, "I will candidly and simply explain how I was delivered from the darkness of Aristotle. When, according to the laws of our university, I had spent three years and a half in the Aristotelian philosophy, and was now invested with the philosophical laurel as a Master of Arts, I took an account of the time which I had consumed in this study; and considered on what subjects I should employ this logical art of Aristotle, which I had learnt with so much labour and noise. I found it made me not more versed in history or antiquities, more eloquent in discourse, more ready in verse, more wise in any subject. Alas for me! how was I overpowered, how deeply did I groan, how did I deplore my lot and my nature, how did I deem myself to be by some unhappy and dismal fate and frame of mind abhorrent from the Muses, when I found that I was one who, after all my pains, could reap no benefit from that wisdom of which I heard so much, as being contained in the Logic of Aristotle." He then relates, that he was led to the study of the Dialogues of Plato, and was delighted with the kind of analysis of the subjects discussed which Socrates is there represented as executing. "Well," he adds, "I began thus to reflect within myself—(I should have thought it impious to say it to another)—What, I pray you, prevents me from *socratizing*; and from asking, without regard to Aristotle's authority, whether Aristotle's Logic be true and correct? It may be that that philosopher leads us wrong; and if so, no wonder that I cannot find in his books the treasure which is not

\* Rami, *Animadversiones Aristotelicæ*, 1. IV.

there. What if his dogmas be mere figments? Do I not tease and torment myself in vain, trying to get a harvest from a barren soil?" He convinced himself that the Aristotelian logic was worthless: and constructed a new system of Logic, founded mainly on the Platonic process of exhausting a subject by analytical classification of its parts. Both works, his *Animadversions on Aristotle*, and his *Logic*, appeared in 1543. The learned world was startled and shocked to find a young man, on his first entrance into life, condemning as faulty, fallacious, and useless, that part of Aristotle's works which had always hitherto been held as a masterpiece of philosophical acuteness, and as the Organon of scientific reasoning. And in truth, it must be granted that Ramus does not appear to have understood the real nature and object of Aristotle's Logic; while his own system could not supply the place of the old one, and was not of much real value. This dissent from the established doctrines was, however, not only condemned but punished. The printing and selling of his books was forbidden through France; and Ramus was stigmatized by a sentence\* which declared him rash, arrogant, impudent, and ignorant, and prohibited from teaching logic and philosophy. He was, however, afterwards restored to the office of professor: and though much attacked, persisted in his plan of reforming, not only Logic but Physics and Metaphysics. He made his position still more dangerous by adopting the reformed religion; and during the unhappy civil wars of France, he was deprived of his professorship, driven from Paris, and had his library plundered. He endeavoured, but in vain, to engage a German professor, Schegk, to undertake the reform of the Aristotelian Physics; a portion of knowledge in which he felt himself not to be strong. Unhappily for himself, he

\* See *Hist. Ind. Sci.*, B. IV. c. iv. sect. 4.

afterwards returned to Paris, where he perished in the massacre of St. Bartholomew in 1572.

Ramus's main objection to the Aristotelian Logic is, that it is not the image of the natural process of thought; an objection which shows little philosophical insight; for the course by which we obtain knowledge may well differ from the order in which our knowledge, when obtained, is exhibited. We have already seen that Ramus's contemporaries, Cæsalpinus and Campanella, had a wiser view; placing definition as the last step in knowing, but the first in teaching. But the effect which Ramus produced was by no means slight. He aided powerfully in turning the minds of men to question the authority of Aristotle on all points; and had many followers, especially among the Protestants. Among the rest, Milton, our great poet, published "*Artis Logicæ plenior Institutio ad Petri Rami methodum concinnata*;" but this work, appearing in 1672, belongs to a succeeding period.

(*The Reformers in general.*)—It is impossible not to be struck with the series of misfortunes which assailed the reformers of philosophy of the period we have had to review. Roger Bacon was repeatedly condemned and imprisoned; and, not to speak of others who suffered under the imputation of magical arts, Telesius is said\* to have been driven from Naples to his native city by calumny and envy; Cæsalpinus was accused of atheism†; Campanella was imprisoned for twenty-seven years and tortured; Giordano Bruno was burnt at Rome as a heretic; Ramus was persecuted during his life, and finally murdered by his personal enemy Jacques Charpentier, in a massacre of which the plea was religion. It is true, that for the most part these misfortunes were not principally due to the attempts at philosophical reform, but were connected rather with politics or religion. But we

\* Tenneman, ix. 200.

† *Ib.* ix. 108.

cannot doubt that the spirit which led men to assail the received philosophy, might readily incline them to reject some tenets of the established religion ; since the boundary line of these subjects is difficult to draw. And as we have seen, there was in most of the persons of whom we have spoken, not only a well-founded persuasion of the defects of existing systems, but an eager spirit of change, and a sanguine anticipation of some wide and lofty philosophy, which was soon to elevate the minds and conditions of men. The most unfortunate were, for the most part, the least temperate and judicious reformers. Patricius, who, as we have seen, declared himself against the Aristotelian philosophy, lived and died at Rome in peace and honour\*.

(*Melancthon.*)—It is not easy to point out with precision the connexion between the efforts at a Reform in Philosophy, and the great Reformation of Religion in the sixteenth century. The disposition to assert (practically at least) a freedom of thinking, and to reject the corruptions which tradition had introduced and authority maintained, naturally extended its influence from one subject to another ; and especially in subjects so nearly connected as theology and philosophy. The Protestants, however, did not reject the Aristotelian system ; they only reformed it, by going back to the original works of the author, and by reducing it to a conformity with Scripture. In this reform, Melancthon was the chief author, and wrote works on Logic, Physics, Morals, and Metaphysics, which were used among Protestants. On the subject of the origin of our knowledge, his views contained a very philosophical improvement of the Aristotelian doctrines. He recognized the importance of Ideas, as well as of Experience. “ We could not,” he says†,

\* Tenneman, ix. 246.

† Melancthon, *De Anima*, p. 207, quoted in Tenneman, ix. 121.

“proceed to reason at all, except there were by nature innate in man certain fixed points, that is, principles of science;—as Number, the recognition of Order and Proportion, logical, geometrical, physical and moral Principles. Physical principles are such as these,—everything which exists proceeds from a cause,—a body cannot be in two places at once,—time is a continued series of things or of motions,—and the like.” It is not difficult to see that such Principles partake of the nature of the Fundamental Ideas which we have attempted to arrange and enumerate in a previous part of this work.

Before we proceed to the next chapter, which treats of the Practical Reformers of Scientific Method, let us for an instant look at the strong persuasion that the time of a philosophical revolution was at hand, implied in the titles of the works of this period. Telesius published *De Rerum Natura juxta propria principia*; Francis Helmont, *Philosophia vulgaris refutata*; Patricius, *Nota de Universis Philosophia*; Campanella, *Philosophia sensibus demonstrata, adversus errores Aristotelis*; Bruno professed himself the author of a *Nolan Philosophy*; and Ramus of a *New Logic*. The age announced itself pregnant; and the eyes of all who took an interest in the intellectual fortunes of the race, were looking eagerly for the expected offspring.

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## CHAPTER X.

### THE PRACTICAL REFORMERS OF SCIENCE.

*Character of the Practical Reformers.*—WE now come to a class of speculators who had perhaps a greater share in bringing about the change from stationary to progressive knowledge, than those writers who so loudly

announced the revolution. The mode in which the philosophers of whom we now speak produced their impressions on men's minds, was very different from the procedure of the theoretical reformers. What these talked of, they did; what these promised, they performed. While the theorists concerning knowledge proclaimed that great advances were to be made, the practical discoverers went steadily forwards. While one class spoke of a complete Reform of scientific Methods, the other, boasting little, and often thinking little of Method, proved the novelty of their instrument by obtaining new results. While the metaphysicians were exhorting men to consult experience and the senses, the physicists were examining nature by such means with unparalleled success. And while the former, even when they did for a moment refer to facts, soon rushed back into their own region of ideas, and tried at once to seize the widest generalizations, the latter, fastening their attention upon the phenomena, and trying to reduce them to laws, were carried forwards by steps measured and gradual, such as no conjectural view of scientific method had suggested; but leading to truths as profound and comprehensive as any which conjecture had dared to anticipate. The theoretical reformers were bold, self-confident, hasty, contemptuous of antiquity, ambitious of ruling all future speculations, as they whom they sought to depose had ruled the past. The practical reformers were cautious, modest, slow, despising no knowledge, whether borrowed from tradition or observation, confident in the ultimate triumph of science, but impressed with the conviction that each single person could contribute a little only to its progress. Yet though thus working rather than speculating,—dealing with particulars more than with generals,—employed mainly in adding to knowledge, and not in defining what knowledge is, or how additions

are to be made to it,—these men, thoughtful, curious, and of comprehensive minds, were constantly led to important views on the nature and methods of science. And these views, thus suggested by reflections on their own mental activity, were gradually incorporated with the more abstract doctrines of the metaphysicians, and had a most important influence in establishing an improved philosophy of science. The indications of such views we must now endeavour to collect from the writings of the discoverers of the times preceding the seventeenth century.

Some of the earliest of these indications are to be found in those who dealt with Art rather than with Science. I have already endeavoured to show that the advance of the arts which give us a command over the powers of nature, is generally prior to the formation of exact and speculative knowledge concerning those powers. But Art, which is thus the predecessor of Science, is, among nations of acute and active intellects, usually its parent. There operates, in such a case, a speculative spirit, leading men to seek for the reasons of that which they find themselves able to do. How slowly, and with what repeated deviations men follow this leading, when under the influence of a partial and dogmatical philosophy, the late birth and slow growth of sound physical theory shows. But at the period of which we now speak, we find men, at length, proceeding in obedience to the impulse which thus drives them from practice to theory;—from an acquaintance with phenomena to a free and intelligent inquiry concerning their causes.

*Leonardo da Vinci.*—I have already noted, in the History of Science, that the Indistinctness of Ideas, which was long one main impediment to the progress of science in the middle ages, was first remedied among architects and engineers. These men, so far at least as



mechanical ideas were concerned, were compelled by their employments to judge rightly of the relations and properties of the materials with which they had to deal; and would have been chastised by the failure of their works, if they had violated the laws of mechanical truth. It was not wonderful, therefore, that these laws became known to them first. We have seen, in the *History*, that Leonardo da Vinci, the celebrated painter, who was also an engineer, is the first writer in whom we find the true view of the laws of equilibrium of the lever in the most general case. This artist, a man of a lively and discursive mind, is led to make some remarks\* on the formation of our knowledge, which may show the opinions on that subject that already offered themselves at the beginning of the sixteenth century†. He expresses himself as follows:—"Theory is the general, Experiments are the soldiers. The interpreter of the artifices of nature is Experience: she is never deceived. Our judgment sometimes is deceived, because it expects effects which Experience refuses to allow." And again, "We must consult Experience, and vary the circumstances till we have drawn from them general rules; for it is she who furnishes true rules. But of what use, you ask, are these rules? I reply, that they direct us in the researches of nature and the operations of art. They prevent our imposing upon ourselves and others, by promising ourselves results which we cannot obtain."

"In the study of the sciences which depend on mathematics, those who do not consult nature but authors, are not the children of nature, they are only her grandchildren. She is the true teacher of men of genius.

\* His works have never been published, and exist in manuscript in the library of the Instituto at Paris. Some extracts were published by Venturi, *Essai sur les Ouvrages de Leonard da Vinci*. Paris, 1797.

† Leonardo died in 1520, at the age of 78.

But see the absurdity of men ! They turn up their noses at a man who prefers to learn from nature herself rather than from authors who are only her clerks."

In another place, in reference to a particular case, he says, " Nature begins from the Reason and ends in Experience ; but for all that, we must take the opposite course ; begin from the Experiment and try to discover the Reason."

Leonardo was born forty-six years before Telesius ; yet we have here an estimate of the value of experience far more just and substantial than the Calabrian school ever reached. The expressions contained in the above extracts, are well worthy our notice ;—that experience is never deceived ;—that we must vary our experiments, and draw from them general rules ;—that nature is the original source of knowledge, and books only a derivative substitute ;—with the lively image of the sons and grandsons of nature. Some of these assertions have been deemed, and not without reason, very similar to those made by Bacon a century later. Yet it is probable that the import of such expressions, in Leonardo's mind, was less clear and definite than that which they acquired by the progress of sound philosophy. When he says that theory is the general and experiments the soldiers, he probably meant that theory directs men what experiments to make ; and had not in his mind the notion of a theoretical Idea ordering and brigading the Facts. When he says that Experience is the interpreter of Nature, we may recollect, that in a more correct use of this image, Experience and Nature are the writing, and the Intellect of man the interpreter. We may add, that the clear apprehension of the importance of Experience led, in this as in other cases, to an unjust depreciation of the value of what science owed to books. Leonardo would have made little progress, if he had attempted to master a complex science,

astronomy for instance, by means of observation alone, without the aid of books.

But in spite of such criticism, Leonardo's maxims show extraordinary sagacity and insight; and they appear to us the more remarkable, when we see how rare such views are for a century after his time.

*Copernicus.*—For we by no means find, even in those practical discoverers to whom, in reality, the revolution in science, and consequently in the philosophy of science, was due, this prompt and vigorous recognition of the supreme authority of observation as a ground of belief; this bold estimate of the probable worthlessness of traditional knowledge; and this plain assertion of the reality of theory founded upon experience. Among such discoverers, Copernicus must ever hold a most distinguished place. The heliocentric theory of the universe, established by him with vast labour and deep knowledge, was, for the succeeding century, the field of discipline and exertion of all the most active speculative minds. Men, during that time, proved their freedom of thought, their hopeful spirit, and their comprehensive view, by adopting, inculcating, and following out the philosophy which this theory suggested. But in the first promulgation of the theory, in the works of Copernicus himself, we find a far more cautious and reserved temper. He does not, indeed, give up the reality of his theory, but he expresses himself so as to avoid shocking those who might (as some afterwards did) think it safe to speak of it as an *hypothesis* rather than a truth. In his preface addressed to the Pope\*, after speaking of the difficulties in the old and received doctrines, by which he was led to his own theory, he says, "Hence I began to think of the mobility of the earth; and although the opinion seemed absurd, yet because I knew that to others before me this liberty had

\* Paul III., in 1543.

been conceded, of imagining any kinds of circles in order to explain the phenomena of the stars, I thought it would also be readily granted me, that I might try whether, by supposing the earth to be in motion, I might not arrive at a better explanation than theirs, of the revolutions of the celestial orbs." Nor does he anywhere assert that the seeming absurdity had become a certain truth, or betray any feeling of triumph over the mistaken belief of his predecessors. And, as I have elsewhere shown, his disciples\* indignantly and justly defended him from the charge of disrespect towards Ptolemy and other ancient astronomers. Yet Copernicus is far from compromising the value or evidence of the great truths which he introduced to general acceptance; and from sinking in his exposition of his discoveries below the temper which had led to them. His quotation from Ptolemy, that "He who is to follow philosophy must be a freeman in mind," is a grand and noble maxim, which it well became him to utter.

*Fabricius.*—In another of the great discoverers of this period, though employed on a very different subject, we discern much of the same temper. Fabricius of Acquapendente†, the tutor and forerunner of our Harvey, and one of that illustrious series of Paduan professors who were the fathers of anatomy‡, exhibits something of the same respect for antiquity, in the midst of his original speculations. Thus in a dissertation§ *On the Action of the Joints*, he quotes Aristotle's Mechanical Problems to prove that in all animal motion there must be some quiescent fulcrum; and finds merit even in Aristotle's ignorance. "Aristotle," he says||, "did not know that motion was produced by the muscle; and after staggering

\* *H. st. Ind. Sci.*, B. v. c. ii.

† Born 1537, died 1619.

‡ *H. st. Ind. Sci.*, B. xvii. c. ii. sect. 1.

§ Fabricius, *De Motu Locali*, p. 182.

|| P. 199.

about from one supposition to another, at last is compelled by the facts themselves to recur to an innate spirit, which, he conceives, is contracted, and which pulls and pushes. And here we cannot help admiring the genius of Aristotle, who, though ignorant of the muscle, invents something which produces nearly the same effect as the muscle, namely, contraction and pulling." He then, with great acuteness, points out the distinction between Aristotle's opinions, thus favourably interpreted, and those of Galen. In all this, we see something of the wish to find all truths in the writings of the ancients, but nothing which materially interferes with freedom of inquiry. The anatomists have in all ages and countries been practically employed in seeking knowledge from observation. Facts have ever been to them a subject of careful and profitable study; while the ideas which enter into the wider truths of the science, are, as we have seen, even still involved in obscurity, doubt, and contest.

*Maurolycus.*—Francis Maurolycus of Messina, whose mathematical works were published in 1575, was one of the great improvers of the science of optics in his time. In his Preface to his Treatise on the Spheres, he speaks of previous writers on the same subject; and observes that as they have not superseded one another, they have not rendered it unfit for any one to treat the subject afresh. "Yet," he says, "it is impossible to amend the errors of all who have preceded us. This would be a task too hard for Atlas, although he supports the heavens. Even Copernicus is tolerated, who makes the sun to be fixed, and the earth to move round it in a circle; and who is more worthy of a whip or a scourge than of a refutation." The mathematicians and astronomers of that time were not the persons most sensible of the progress of physical knowledge; for the bases of their science, and a great part of its substance, were contained in the

writings of the ancients; and till the time of Kepler, Ptolemy's work was, very justly, looked upon as including all that was essential in the science.

*Benedetti.*—But the writers on Mechanics were naturally led to present themselves as innovators and experimenters; for all that the ancients had taught concerning the doctrine of motion was erroneous; while those who sought their knowledge from experiment, were constantly led to new truths. John Baptist Benedetti, a Venetian nobleman, in 1599, published his *Speculationum Liber*, containing, among other matter, a treatise on Mechanics, in which several of the Aristotelian errors were refuted. In the Preface to this Treatise, he says, "Many authors have written much, and with great ability, on Mechanics; but since nature is constantly bringing to light something either new, or before unnoticed, I too wished to put forth a few things hitherto unattempted, or not sufficiently explained." In the doctrine of motion he distinctly and at some length condemns and argues against all the Aristotelian doctrines concerning motion, weight, and many other fundamental principles of physics. Benedetti is also an adherent of the Copernican doctrine. He states\* the enormous velocity which the heavenly bodies must have, if the earth be the centre of their motions; and adds, "which difficulty does not occur according to the beautiful theory of the Samian Aristarchus, expounded in a divine manner by Nicolas Copernicus; against which the reasons alleged by Aristotle are of no weight." Benedetti throughout shows no want of the courage or ability which were needed in order to rise in opposition against the dogmas of the Peripatetics. He does not, however, refer to experiment in a very direct manner; indeed most of the facts on which the elementary truths of mechanics

\* *Speculationum Liber*, p. 195.

rest, were known and admitted by the Aristotelians; and therefore could not be adduced as novelties. On the contrary, he begins with *a priori* maxims, which experience would not have confirmed. "Since," he says\*, "we have undertaken the task of proving that Aristotle is wrong in his opinions concerning motion, there are certain absolute truths, the objects of the intellect known of themselves, which we must lay down in the first place." And then, as an example of these truths, he states this: "Any two bodies of equal size and figure, but of different materials, will have their natural velocities in the same proportion as their weights;" where by their natural velocities, he means the velocities with which they naturally fall downwards.

*Gilbert.*—The greatest of these practical reformers of science is our countryman, William Gilbert; if, indeed, in virtue of the clear views of the prospects which were then opening to science, and of the methods by which her future progress was to be secured, while he exemplified those views by physical discoveries, he do not rather deserve the still higher praise of being at the same time a theoretical and a practical reformer. Gilbert's physical researches and speculations were employed principally upon subjects on which the ancients had known little or nothing; and on which therefore it could not be doubtful whether tradition or observation was the source of knowledge. Such was magnetism; for the ancients were barely acquainted with the attractive property of the magnet. Its polarity, including repulsion as well as attraction, its direction towards the north, its limited variation from this direction, its declination from the horizontal position, were all modern discoveries. Gilbert's work† on the magnet and on the magnetism of

\* *Speculationum Liber*, p. 169.

† Gulielmi Gilberti, *Colcestriensis, Medici Londinensis, De Mag-*

the earth, appeared in 1600; and in this, he repeatedly maintains the superiority of experimental knowledge over the physical philosophy of the ancients. His preface opens thus: "Since in making discoveries and searching out the hidden causes of things, stronger reasons are obtained from trustworthy experiments and demonstrable arguments, than from probable conjectures and the dogmas of those who philosophize in the usual manner," he has, he says, "endeavoured to proceed from common magnetical experiments to the inward constitution of the earth." As I have stated in the *History of Magnetism*\*, Gilbert's work contains all the fundamental facts of that science, so fully stated, that we have, at this day, little to add to them. He is not, however, by the advance which he thus made, led to depreciate the ancients, but only to claim for himself the same liberty of philosophizing which they had enjoyed†. "To those ancient and first parents of philosophy, Aristotle, Theophrastus, Ptolemy, Hippocrates, Galen, be all due honour; from them it was that the stream of wisdom has been derived down to posterity. But our age has discovered and brought to light many things which they, if they were yet alive, would gladly embrace. Wherefore we also shall not hesitate to expound, by probable hypotheses, those things which by long experience we have ascertained."

In this work the author not only adopts the Copernican doctrine of the earth's motion, but speaks‡ of the contrary supposition as utterly absurd, founding his argument mainly on the vast velocities which such a supposition requires us to ascribe to the celestial bodies. Dr. Gilbert was physician to Queen Elizabeth and to James

*nete, Magneticisque Corporibus, et de Magno Magnete Tellure, Physiologia Nova, plurimis et Argumentis et Experimentis demonstrata.*

\* *Hist. Ind. Sci.*, B. XII. c. i.

† Pref.

‡ *De Magnete*, Lib. vi. c. 3, 4.



the First, and died in 1603. Sometime after his death the executors of his brother published another work of his, *De Mundo nostro Sublunari Philosophia Nova*, in which similar views are still more comprehensively presented. In this he says, "The two lords of philosophy, Aristotle and Galen, are held in worship like gods, and rule the schools;—the former by some destiny obtained a sway and influence among philosophers, like that of his pupil Alexander among the kings of the earth;—Galen, with like success, holds his triumph among the physicians of Europe." This comparison of Aristotle to Alexander was also taken hold of by Bacon. Nor is Gilbert an unworthy precursor of Bacon in the view he gives of the History of Science, which occupies the first three chapters of his Philosophy. He traces this history from "the simplicity and ignorance of the ancients," through "the fabrication of the fable of the four elements," to Aristotle and Galen. He mentions with due disapproval the host of commentators which succeeded, the alchemists, the "shipwreck of science in the deluge of the Goths," and the revival of letters and genius in the time of "our grandfathers." "This later age," he says, "has exploded the Barbarians, and restored the Greeks and Latins to their pristine grace and honour. It remains, that if they have written aught in error, this should be remedied by better and more productive processes (*frugiferis institutis*,) not to be contemned for their novelty; (for nothing which is true is really new, but is perfect from eternity, though to weak man it may be unknown;) and that thus Philosophy may bear her fruit." The reader of Bacon will not fail to recognize, in these references to "fruit-bearing" knowledge, a similarity of expression with the *Novum Organon*.

Bacon does not appear to me to have done justice to his contemporary. He nowhere recognizes in the labours

of Gilbert a community of purpose and spirit with his own. On the other hand, he casts upon him a reflection which he by no means deserves. In the *Advancement of Learning*\*, he says, "Another error is, that men have used to infect their meditations, opinions, and doctrines, with some conceits which they have most admired, or some sciences to which they have most applied; and given all things else a tincture according to them, utterly untrue and improper. . . . So have the alchemists made a philosophy out of a few experiments of the furnace; and Gilbertus, our countryman, hath made a philosophy out of the observations of a loadstone," (in the Latin, philosophiam etiam e magnete elicuit.) And in the same manner he mentions him in the *Novum Organon*†, as affording an example of an empirical kind of philosophy, which appears to those daily conversant with the experiments, probable, but to other persons incredible and empty. But instead of blaming Gilbert for disturbing and narrowing science by a too constant reference to magnetical rules, we might rather censure Bacon, for not seeing how important in all natural philosophy are those laws of attraction and repulsion of which magnetical phenomena are the most obvious illustration. We may find ground for such a judgment in another passage in which Bacon speaks of Gilbert. In the Second Book‡ of the *Novum Organon*, having classified motions, he gives, as one kind, what he calls, in his figurative language, *motion for gain*, or *motion of need*, by which a body shuns heterogeneous, and seeks cognate bodies. And he adds, "The Electrical operation, concerning which Gilbert and others since him have made up such a wonderful story, is nothing less than the appetite of a body, which, excited by friction, does not well tolerate the air, and prefers another tangible body if it be found

\* *Nov. Org.*, Book 1.    † Book 1. Aph. 64.    ‡ Vol. ix. 185.

near." Bacon's notion of an appetite in the body is certainly much less philosophical than Gilbert's, who speaks of light bodies as drawn towards amber by certain material radii\*; and we might perhaps venture to say that Bacon here manifests a want of clear mechanical ideas. Bacon, too, showed his inferior aptitude for physical research in rejecting the Copernican doctrine which Gilbert adopted. In the *Advancement of Learning*†, suggesting a history of the opinions of philosophers, he says that he would have inserted in it even recent theories, as those of Paracelsus; of Telesius, who restored the philosophy of Parmenides; or Patricius, who resublimed the fumes of Platonism; or Gilbert, who brought back the dogmas of Philolaus. But Bacon quotes‡ with pleasure Gilbert's ridicule of the Peripatetics' definition of heat. They had said, that heat is that which separates heterogeneous and unites homogeneous matter; which, said Gilbert, is as if any one were to define *man* as that which sows wheat and plants vines.

Galileo, another of Gilbert's distinguished contemporaries, had a higher opinion of him. He says§, "I extremely admire and envy this author. I think him worthy of the greatest praise for the many new and true observations which he has made, to the disgrace of so many vain and fabling authors; who write, not from their own knowledge only, but repeat everything they hear from the foolish and vulgar, without attempting to satisfy themselves of the same by experience; perhaps that they may not diminish the size of their books."

*Galileo.*—Galileo was content with the active and successful practice of experimental inquiry; and did not demand that such researches should be made expressly

\* *De Magnete*, p. 60.

† Book III. c. 4.

‡ *Nov. Org.*, Book II. Aph. 48.

§ Drinkwater's *Life of Galileo*, p. 18.

subservient to that wider and more ambitious philosophy, on which the author of the *Novum Organon* employed his powers. But still it now becomes our business to trace those portions of Galileo's views which have reference to the theory, as well as the practice, of scientific investigation. On this subject, Galileo did not think more profoundly, perhaps, than several of his contemporaries; but in the liveliness of expression and illustration with which he recommended his opinions on such topics, he was unrivalled. Writing in the language of the people, in the attractive form of dialogue, with clearness, grace, and wit, he did far more than any of his predecessors had done to render the new methods, results, and prospects of science familiar to a wide circle of readers, first in Italy, and soon, all over Europe. The principal points inculcated by him were already becoming familiar to men of active and inquiring minds; such as,—that knowledge was to be sought from observation, and not from books;—that it was absurd to adhere to, and debate about, the physical tenets of Aristotle and the rest of the ancients. On persons who followed this latter course, Galileo fixed the epithet of Paper Philosophers\*; because, as he wrote in a letter to Kepler, this sort of men fancied that philosophy was to be studied like the *Æneid* or *Odyssee*, and that the true reading of nature was to be detected by the collation of texts. Nothing so much shook the authority of the received system of Physics as the experimental discoveries, directly contradicting it, which Galileo made. By experiment, as I have elsewhere stated†, he disproved the Aristotelian doctrine that bodies fall quickly or slowly in proportion to their weight. And when he had invented the telescope, a number of new discoveries of the most striking kind (the inequalities of the moon's

\* *Life of Galileo*, p. 9.

† *Hist. Ind. Sci.*, B. VI. c. ii. sect. 5.

surface, the spots in the sun, the moon-like phases of Venus, the satellites of Jupiter, the ring of Saturn,) showed, by the evidence of the eyes, how inadequate were the conceptions, and how erroneous the doctrines of the ancients, respecting the constitution of the universe. How severe the blow was to the disciples of the ancient schools, we may judge by the extraordinary forms of defence in which they tried to intrench themselves. They would not look through Galileo's glasses; they maintained that what was seen was an illusion of witchcraft; and they tried, as Galileo says\*, with logical arguments, as if with magical incantations, to charm the new planets out of the sky. No one could be better fitted than Galileo for such a warfare. His great knowledge, clear intellect, gaiety, and light irony, (with the advantage of being in the right,) enabled him to play with his adversaries as he pleased. Thus when an Aristotelian† rejected the discovery of the irregularities in the moon's surface, because, according to the ancient doctrine, her form was a perfect sphere, and held that the apparent cavities were filled with an invisible crystal substance; Galileo replied, that he had no objection to assent to this, but that then he should require his adversary in return to believe that there were on the same surface invisible crystal mountains ten times as high as those visible ones which he had actually observed and measured.

We find in Galileo many thoughts which have since become established maxims of modern philosophy. "Philosophy," he says‡, "is written in that great book, I mean the Universe, which is constantly open before our eyes; but it cannot be understood, except we first know the language and learn the characters in which it is written." With this thought he combines some other

\* *Life of Galileo*, p. 29.    + *Ib.*, p. 33.    ‡ *Il Saggiatore*, II. 247.

lively images. One of his interlocutors says concerning another, "Sarsi perhaps thinks that philosophy is a book made up of the fancies of men, like the *Iliad* or *Orlando Furioso*, in which the matter of least importance is, that what is written be true." And again, with regard to the system of authority, he says, "I think I discover in him a firm belief that, in philosophizing, it is necessary to lean upon the opinion of some celebrated author; as if our mind must necessarily remain unfruitful and barren till it be married to another man's reason."—"No," he says, "the case is not so.—When we have the decrees of Nature, authority goes for nothing; reason is absolute\*."

In the course of Galileo's controversies, questions of the logic of science came under discussion. Vincenzio di Grazia objected to a proof from induction which Galileo adduced, because *all* the particulars were not enumerated; to which the latter justly replies†, that if induction were required to pass through all the cases, it would be either useless or impossible;—impossible when the cases are innumerable; useless when they have each already been verified, since then the general proposition adds nothing to our knowledge.

One of the most novel of the characters which Science assumes in Galileo's hands is, that she becomes cautious. She not only proceeds leaning upon Experience, but she is content to proceed a little way at a time. She already begins to perceive that she must rise to the heights of knowledge by many small and separate steps. The philosopher is desirous to know much, but resigned to be ignorant for a time of that which cannot yet be known. Thus when Galileo discovered the true law of the motion of a falling body‡, that the velocity increases proportionally to the time from the beginning of the fall, he did not

\* *Il Saggiatore*, II. 200.

† *Ib.*, I. 501.

‡ *Hist. Ind. Sci.*, B. VI. c. II. sect. 2.

insist upon immediately assigning the cause of this law. "The cause of the acceleration of the motions of falling bodies is not," he says, "a necessary part of the investigation." Yet the conception of this acceleration, as the result of the continued action of the force of gravity upon the falling body, could hardly fail to suggest itself to one who had formed the idea of force. In like manner, the truth that the velocities, acquired by bodies falling down planes of equal heights, are all equal, was known to Galileo and his disciples, long before he accounted for it\*, by the principle, apparently so obvious, that the momentum generated is as the moving force which generates it. He was not tempted to rush at once, from an experimental truth to a universal system. Science had learnt that she must move step by step; and the gravity of her pace already indicated her approaching maturity and her consciousness of the long path which lay before her.

But besides the genuine philosophical prudence which thus withheld Galileo from leaping hastily from one inference to another, he had perhaps a preponderating inclination towards facts; and did not feel, so much as some other persons of his time, the need of reducing them to ideas. He could bear to contemplate laws of motion without being urged by an uncontrollable desire to refer them to conceptions of force.

*Kepler.*—In this respect his friend Kepler differed from him; for Kepler was restless and unsatisfied till he had reduced facts to laws, and laws to causes; and never acquiesced in ignorance, though he tested with the most rigorous scrutiny that which presented itself in the shape of knowledge to fill the void. It may be seen in the History of Astronomy† with what perseverance, energy, and fertility of invention, Kepler pursued his labours,

\* *Hist. Ind. Sci.*, B. vi. c. ii. sect. 4.      † *Ib.*, B. v. c. iv. sect. 1.

(enlivened and relieved by the most curious freaks of fancy,) with a view of discovering the rules which regulate the motions of the planet Mars. He represents this employment under the image of a warfare; and describes\* his object to be "to triumph over Mars, and to prepare for him, as for one altogether vanquished, tabular prisons and equated eccentric fetters;" and when "the enemy, left at home a despised captive, had burst all the chains of the equations, and broken forth of the prisons of the tables;"—when "it was buzzed here and there that the victory is vain, and that the war is raging anew as violently as before;"—that is, when the rules which he had proposed did not coincide with the facts;—he by no means desisted from his attempts, but "suddenly sent into the field a reserve of new physical reasonings on the rout and dispersion of the veterans," that is, tried new suppositions suggested by such views as he then entertained of the celestial motions. His efforts to obtain the formal laws of the planetary motions resulted in some of the most important discoveries ever made in astronomy; and if his physical reasonings were for the time fruitless, this arose only from the want of that discipline in mechanical ideas which the minds of mathematicians had still to undergo; for the great discoveries of Newton in the next generation showed that, in reality, the next step of the advance was in this direction. Among all Kepler's fantastical expressions, the fundamental thoughts were sound and true; namely, that it was his business, as a physical investigator, to discover a mathematical rule which governed and included all the special facts; and that the rules of the motions of the planets must conform to some conception of causation.

The same characteristics,—the conviction of rule and cause, perseverance in seeking these, inventiveness in

\* *De Stell. Mart.*, p. iv. c. 51. (1609.) Drinkwater's *Kepler*, p. 33.



devising hypotheses, love of truth in trying and rejecting them, and a lively Fancy playing with the Reason without interrupting her,—appear also in his work on Optics; in which he tried to discover the exact law of optical refraction\*. In this undertaking he did not succeed entirely; nor does he profess to have done so. He ends his numerous attempts by saying, “Now, reader, you and I have been detained sufficiently long while I have been attempting to *collect into one fagot* the measures of different refractions.”

In this and in other expressions, we see how clearly he apprehended that *colligation of facts* which is the main business of the practical discoverer. And by his peculiar endowments and habits, Kepler exhibits an essential portion of this process, which hardly appears at all in Galileo. In order to bind together facts, theory is requisite as well as observation,—the cord as well as the fagots. And the true theory is often, if not always, obtained by trying several and selecting the right. Now of this portion of the discoverer's exertions, Kepler is a most conspicuous example. His fertility in devising suppositions, his undaunted industry in calculating the results of them, his entire honesty and candour in resigning them if these results disagreed with the facts, are a very instructive spectacle; and are fortunately exhibited to us in the most lively manner in his own garrulous narratives. Galileo urged men by precept as well as example to begin their philosophy from observation; Kepler taught them by his practice that they must proceed from observation by means of hypotheses. The one insisted upon facts; the other dealt no less copiously with ideas. In the practical, as in the speculative portion of our history, this antithesis shows itself; although in the practical part we cannot have the two

\* Published 1604. *Hist. Ind. Sci.*, B. ix. c. ii.

elements separated, as in the speculative we sometimes have.

In the *History of Science*\*, I have devoted several pages to the intellectual character of Kepler, inasmuch as his habit of devising so great a multitude of hypotheses, so fancifully expressed, had led some writers to look upon him as an inquirer who transgressed the most fixed rules of philosophical inquiry. This opinion has arisen, I conceive, among those who have forgotten the necessity of Ideas as well as Facts for all theory; or who have overlooked the impossibility of selecting and explicating our ideas without a good deal of spontaneous play of the mind. It must, however, always be recollected that Kepler's genius and fancy derived all their scientific value from his genuine and unmingled love of truth. These qualities appeared, not only in the judgment he passed upon hypotheses, but also in matters which more immediately concerned his reputation. Thus when Galileo's discovery of the telescope disproved several opinions which Kepler had published and strenuously maintained, he did not hesitate a moment to retract his assertions and range himself by the side of Galileo, whom he vigorously supported in his warfare against those who were incapable of thus cheerfully acknowledging the triumph of new facts over their old theories.

*Tycho*.—There remains one eminent astronomer, the friend and fellow-labourer of Kepler, whom we must not separate from him as one of the practical reformers of science. I speak of Tycho Brahe, who is, I think, not justly appreciated by the literary world in general, in consequence of his having made a retrograde step in that portion of astronomical theory which is most familiar to the popular mind. Though he adopted the

\* *Hist. Ind. Sci.*, B. v. c. iv. sect. 1.

Copernican view of the motion of the planets about the sun, he refused to acknowledge the annual and diurnal motion of the earth. But notwithstanding this mistake, into which he was led by his interpretation of Scripture rather than of nature, Tycho must ever be one of the greatest names in astronomy. In the philosophy of science also, the influence of what he did is far from inconsiderable; and especially its value in bringing into notice these two points:—that not only are observations the beginning of science, but that the progress of science may often depend upon the observer's pursuing his task regularly and carefully for a long time, and with well devised instruments; and again, that observed facts offer a *succession* of laws which we discover as our observations become better, and as our theories are better adapted to the observations. With regard to the former point, Tycho's observatory was far superior to all that had preceded it\*, not only in the optical, but in the mechanical arrangements; a matter of almost equal consequence. And hence it was that his observations inspired in Kepler that confidence which led him to all his labours and all his discoveries. "Since," he says†, "the divine goodness has given us in Tycho Brahe an exact observer, from whose observations this error of eight minutes in the calculations of the Ptolemaic hypothesis is detected, let us acknowledge and make use of this gift of God: and since this error cannot be neglected, these eight minutes alone have prepared the way for an entire reform of Astronomy, and are to be the main subject of this work."

With regard to Tycho's discoveries respecting the moon, it is to be recollected that besides the first inequality of the moon's motion, (the *equation of the center*, arising from the elliptical form of her orbit,) Ptolemy

\* *Hist. Ind. Sci.*, B. VII. c. vi. sect. 1. † *De Stell. Mart.*, p. 11, c. 19.

had discovered a second inequality, the *evection*, which, as we have observed in the History of this subject\*, might have naturally suggested the suspicion that there were still other inequalities. In the middle ages, however, such suggestions, implying a constant progress in science, were little attended to; and, we have seen, that when an Arabian astronomer† had really discovered another inequality of the moon, it was soon forgotten, because it had no place in the established systems. Tycho not only rediscovered the lunar inequality, (the *variation*,) thus once before won and lost, but also two other inequalities; namely‡, the *change of inclination* of the moon's orbit as the line of nodes moves round, and an inequality in the motion of the line of nodes. Thus, as I have elsewhere said, it appeared that the discovery of a rule is a step to the discovery of deviations from that rule, which require to be expressed in other rules. It became manifest to astronomers, and through them to all philosophers, that in the application of theory to observation, we find, not only the stated phenomena, for which the theory does account, but also *residual phenomena*, which are unaccounted for, and remain over and above the calculation. And it was seen further, that these residual phenomena might be, altogether or in part, exhausted by new theories.

These were valuable lessons; and the more valuable inasmuch as men were now trying to lay down maxims and methods for the conduct of science. A revolution was not only at hand, but had really taken place, in the great body of real cultivators of science. The occasion now required that this revolution should be formally recognized;—that the new intellectual power should be clothed with the forms of government;—that the new

\* *Hist. Ind. Sci.*, B. II. c. iv. sect. 6.

† *Ib.*, sect. 8.

‡ Montucla, i. 566.

philosophical republic should be acknowledged as a sister state by the ancient dynasties of Aristotle and Plato. There was needed some great Theoretical Reformer, to speak in the name of the Experimental Philosophy; to lay before the world a declaration of its rights and a scheme of its laws. And thus our eyes are turned to Francis Bacon, and others who like him attempted this great office. We quit those august and venerable names of discoverers, whose appearance was the prelude and announcement of the new state of things then opening; and in doing so, we may apply to them the language which Bacon applies to himself\* :—

Χαίρετε Κήρυκες Διὸς ἄγγελοι ἡδὲ καὶ ἀνδρῶν.  
Hail Heralds, Messengers of Gods and Men !

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## CHAPTER XI.

### FRANCIS BACON.

1. It is a matter of some difficulty to speak of the character and merits of this illustrious man, as regards his place in that philosophical history with which we are here engaged. If we were to content ourselves with estimating him according to the office which, as we have just seen, he claims for himself†, as merely the harbinger and announcer of a sounder method of scientific inquiry than that which was recognized before him, the task would be comparatively easy. For we might select from his writings those passages in which he has delivered opinions and pointed out processes, then novel and strange, but since confirmed by the experience of actual discoverers, and by the judgments of the wisest of suc-

\* *De Augm.*, Lib. iv. c. 1.

† And in other passages: thus, "Ego enim buccinator tantum pugnam non inco." *Nov. Org.*, Lib. iv. c. 1.

ceeding philosophers; and we might pass by, without disrespect, but without notice, maxims and proposals which have not been found available for use;—views so indistinct and vague, that we are even yet unable to pronounce upon their justice;—and boundless anticipations, dictated by the sanguine hopes of a noble and comprehensive intellect. But if we thus reduce the philosophy of Bacon to that portion which the subsequent progress of science has rigorously verified, we shall have to pass over many of those declarations which have excited most notice in his writings, and shall lose sight of many of those striking thoughts which his admirers most love to dwell upon. For he is usually spoken of, at least in this country, as a teacher who not only commenced, but in a great measure completed, the Philosophy of Induction. He is considered, not only as having asserted some general principles, but laid down the special rules of scientific investigation; as not only one of the Founders, but the supreme Legislator of the modern Republic of Science; not only the Hercules who slew the monsters that obstructed the earlier traveller, but the Solon who established a constitution fitted for all future time.

2. Nor is it our purpose to deny that of such praise he deserves a share which, considering the period at which he lived, is truly astonishing. But it is necessary for us in this place to discriminate and select that portion of his system which, bearing upon *physical* science, has since been confirmed by the actual history of science. Many of Bacon's most impressive and captivating passages contemplate the extension of the new methods of discovering truth to intellectual, to moral, to political, as well as to physical science. And how far, and how, the advantages of the inductive method may be secured for those important branches of speculation, it will at

some future time be a highly interesting task to examine. But our plan requires us at present to omit the consideration of these; for our purpose is to learn what the genuine course of the formation of science is, by tracing it in those portions of human knowledge, which, by the confession of all, are most exact, most certain, most complete. Hence we must here deny ourselves the dignity and interest which float about all speculations in which the great moral and political concerns of men are involved. It cannot be doubted that the commanding position which Bacon occupies in men's estimation arises from his proclaiming a reform in philosophy of so comprehensive a nature;—a reform which was to infuse a new spirit into every part of knowledge. Physical Science has tranquilly and noiselessly adopted many of his suggestions; which were, indeed, her own natural impulses, not borrowed from him; and she is too deeply and satisfactorily absorbed in contemplating her results, to talk much about the methods of obtaining them which she has thus instinctively pursued. But the philosophy which deals with mind, with manners, with morals, with polity, is conscious still of much obscurity and perplexity; and would gladly borrow aid from a system in which aid is so confidently promised. The aphorisms and phrases of the *Novum Organon* are far more frequently quoted by metaphysical, ethical, and even theological writers, than they are by the authors of works on physics.

3. Again, even as regards physics, Bacon's fame rests upon something besides the novelty of the maxims which he promulgated. That a revolution in the method of scientific research was going on, all the greatest physical investigators of the sixteenth century were fully aware, as we have shown in the last chapter. But their writings conveyed this conviction to the public at large somewhat slowly. Men of letters, men of the world, men

of rank, did not become familiar with the abstruse works in which these views were published; and above all, they did not, by such occasional glimpses as they took of the state of physical science, become aware of the magnitude and consequences of this change. But Bacon's lofty eloquence, wide learning, comprehensive views, bold pictures of the coming state of things, were fitted to make men turn a far more general and earnest gaze upon the passing change. When a man of his acquirements, of his talents, of his rank and position, of his gravity and caution, poured forth the strongest and loftiest expressions and images which his mind could supply, in order to depict the "Great Instauration" which he announced;—in order to contrast the weakness, the blindness, the ignorance, the wretchedness, under which men had laboured while they followed the long beaten track, with the light, the power, the privileges, which they were to find in the paths to which he pointed;—it was impossible that readers of all classes should not have their attention arrested, their minds stirred, their hopes warmed; and should not listen with wonder and with pleasure to the strains of prophetic eloquence in which so great a subject was presented. And when it was found that the prophecy was verified; when it appeared that an immense change in the methods of scientific research really *had* occurred;—that vast additions to man's knowledge and power had been acquired, in modes like those which had been spoken of;—that further advances might be constantly looked for;—and that a progress, seemingly boundless, was going on in the direction in which the seer had thus pointed;—it was natural that men should hail him as the leader of the revolution; that they should identify him with the event which he was the first to announce; that they should look upon him as the author of that



which he had, as they perceived, so soon and so thoroughly comprehended.

4. For we must remark, that although (as we have seen) he was not the only, nor the earliest writer, who declared that the time was come for such a change, he not only proclaimed it more emphatically, but understood it, in its general character, much more exactly, than any of his contemporaries. Among the maxims, suggestions and anticipations which he threw out, there were many of which the wisdom and the novelty were alike striking to his immediate successors;—there are many which even now, from time to time, we find fresh reason to admire, for their acuteness and justice. Bacon stands far above the herd of loose and visionary speculators who, before and about his time, spoke of the establishment of new philosophies. If we must select some one philosopher as the Hero of the revolution in scientific method, beyond all doubt Francis Bacon must occupy the place of honour.

We shall, however, no longer dwell upon these general considerations, but shall proceed to notice some of the more peculiar and characteristic features of Bacon's philosophy; and especially those views, which, occurring for the first time in his writings, have been fully illustrated and confirmed by the subsequent progress of science, and have become a portion of the permanent philosophy of our times.

5. (I.) The first great feature which strikes us in Bacon's philosophical views is that which we have already noticed;—his confident and emphatic announcement of a *New Era* in the progress of science, compared with which the advances of former times were poor and trifling. This was with Bacon no loose and shallow opinion, taken up on light grounds and involving only vague general notions. He had satisfied himself of the justice

of such a view by a laborious course of research and reflection. In 1605, at the age of forty-four, he published his *Treatise of the Advancement of Learning*, in which he takes a comprehensive and spirited survey of the condition of all branches of knowledge which had been cultivated up to that time. This work was composed with a view to that reform of the existing philosophy which Bacon always had before his eyes; and in the Latin edition of his works, forms the First Part of the *Instauratio Magna*. In the Second Part of the *Instauratio*, the *Novum Organon*, published in 1620, he more explicitly and confidently states his expectations on this subject. He points out how slightly and feebly the examination of nature had been pursued up to his time, and with what scanty fruit. He notes the indications of this in the very limited knowledge of the Greeks who had till then been the teachers of Europe, in the complaints of authors concerning the subtilty and obscurity of the secrets of nature, in the dissensions of sects, in the absence of useful inventions resulting from theory, in the fixed form which the sciences had retained for two thousand years. Nor, he adds\*, is this wonderful; for how little of his thought and labour has man bestowed upon science! Out of twenty-five centuries scarce six have been favourable to the progress of knowledge. And even in those favoured times, natural philosophy received the smallest share of man's attention; while the portion so given was marred by controversy and dogmatism; and even those who have bestowed a little thought upon this philosophy, have never made it their main study, but have used it as a passage or drawbridge to serve other objects. And thus, he says, the great Mother of the Sciences is thrust down with indignity to the offices of a handmaid; is made to minister to the labours of

\* Lib. I. Aphor. 78 *et seq.*

medicine or mathematics, or to give the first preparatory tinge to the immature minds of youth. For these and similar considerations of the errors of past time, he draws hope for the future, employing the same argument which Demosthenes uses to the Athenians: "That which is worst in the events of the past, is the best as a ground of trust in the future. For if you had done all that became you, and still had been in this condition, your case might be desperate; but since your failure is the result of your own mistakes, there is good hope that, correcting the error of your course, you may reach a prosperity yet unknown to you."

6. (II.) All Bacon's hope of improvement indeed was placed in an entire *change of the Method* by which science was pursued; and the boldness, and at the same time, (the then existing state of science being considered) the definiteness of his views of the change that was requisite are truly remarkable.

That all knowledge must begin with observation, is one great principle of Bacon's philosophy; but I hardly think it necessary to notice the inculcation of this maxim as one of his main services to the cause of sound knowledge, since it had, as we have seen, been fully insisted upon by others before him, and was growing rapidly into general acceptance without his aid. But if he was not the first to tell men that they must collect their knowledge from observation, he had no rival in his peculiar office of teaching them *how* science must thus be gathered from experience.

It appears to me that by far the most extraordinary parts of Bacon's works are those in which, with extreme earnestness and clearness, he insists upon a *graduated and successive induction*, as opposed to a hasty transit from special facts to the highest generalizations. The nineteenth Axiom of the First Book of the *Novum Organon*

contains a view of the nature of true science most exact and profound; and, so far as I am aware, at the time perfectly new. "There are two ways, and can only be two, of seeking and finding truth. The one, from sense and particulars, takes a flight to the most general axioms, and from those principles and their truth, settled once for all, invents and judges of intermediate axioms. The other method collects axioms from sense and particulars, ascending *continuously and by degrees*, so that in the end it arrives at the most general axioms; this latter way is the true one, but hitherto untried."

It is to be remarked, that in this passage Bacon employs the term *axioms* to express any propositions collected from facts by induction, and thus fitted to become the starting-point of deductive reasonings. How far propositions so obtained may approach to the character of axioms in the more rigorous sense of the term, we have already in some measure examined; but that question does not here immediately concern us. The truly remarkable circumstance is to find this recommendation of a continuous advance from observation, by limited steps, through successive gradations of generality, given at a time when speculative men in general had only just begun to perceive that they must begin their course from experience in some way or other. How exactly this description represents the general structure of the soundest and most comprehensive physical theories, all persons who have studied the progress of science up to modern times can bear testimony; but perhaps this structure of science cannot in any other way be made so apparent as by those Tables of successive generalizations in which we have exhibited the history and constitution of some of the principal physical sciences, in the Chapter of the preceding Book which treats of the Logic of Induction. And the view which Bacon thus took of the

true progress of science was not only new, but, so far as I am aware, has never been adequately illustrated up to the present day.

7. It is true, as I observed in the last chapter, that Galileo had been led to see the necessity, not only of proceeding from experience in the pursuit of knowledge, but of proceeding cautiously and gradually; and he had exemplified this rule more than once, when, having made one step in discovery, he held back his foot, for a time, from the next step, however tempting. But Galileo had not reached this wide and commanding view of the successive subordination of many steps, all leading up at last to some wide and simple general truth. In catching sight of this principle, and in ascribing to it its due importance, Bacon's sagacity, so far as I am aware, wrought unassisted and unrivalled.

8. Nor is there any wavering or vagueness in Bacon's assertion of this important truth. He repeats it over and over again; illustrates it by a great number of the most lively metaphors and emphatic expressions. Thus he speaks of the successive *floors (tabulata)* of induction; and speaks of each science as a *pyramid*\* which has observation and experience for its basis. No images can better exhibit the relation of general and particular truths, as our own Inductive Tables may serve to show.

9. (III.) Again; not less remarkable is his contrasting this true Method of Science (while it was almost, as he says, yet untried) with the ancient and *vicious Method*, which began, indeed, with facts of observation, but rushed

\* *Aug. Sc.*, Lib. III. c. 4. p. 194. So in other places, as *Nov. Org.*, I. Aphorism 104. "De scientiis tum demum bene sperandum est quando per scalam veram et per gradus continuos, et non intermissos aut hiulcos a particularibus ascendetur ad axiomata minora, et deinde ad media, alia aliis superiora, et postremo demum ad generalissima."

at once, and with no gradations, to the most general principles. For this was the course which had been actually followed by all those speculative reformers who had talked so loudly of the necessity of beginning our philosophy from experience. All these men, if they attempted to frame physical doctrines at all, had caught up a few facts of observation, and had erected a universal theory upon the suggestions which these offered. This process of illicit generalization, or, as Bacon terms it, Anticipation of Nature (*anticipatio naturæ*), in opposition to the Interpretation of Nature, he depicts with singular acuteness, in its character and causes. "These two ways," he says \* "both begin from sense and particulars; but their discrepancy is immense. The one merely skims over experience and particulars in a cursory transit; the other deals with them in a due and orderly manner. The one, at its very outset, frames certain general abstract principles, but useless; the other gradually rises to those principles which have a real existence in nature."

"The former path," he adds†, "that of illicit and hasty generalization, is one which the intellect follows when abandoned to its own impulse; and this it does from the requisitions of logic. For the mind has a yearning which makes it dart forth to generalities, that it may have something to rest in; and after a little dallying with experience, becomes weary of it; and all these evils are augmented by logic, which requires these generalities to make a show with in its disputations."

"In a sober, patient, grave intellect," he further adds, "the mind, by its own impulse, (and more especially if it be not impeded by the sway of established opinions) attempts in some measure that other and true way, of gradual generalization; but this it does with small profit;

\* *Nov. Org.*, I. Aph. 22.

† *Ib.*, Aph. 20.

for the intellect, except it be regulated and aided, is a faculty of unequal operation, and altogether unapt to master the obscurity of things."

The profound and searching wisdom of these remarks appears more and more, as we apply them to the various attempts which men have made to obtain knowledge; when they begin with the contemplation of a few facts, and pursue their speculations, as upon most subjects they have hitherto generally done; for almost all such attempts have led immediately to some process of illicit generalization, which introduces an interminable course of controversy. In the physical sciences, however, we have the further inestimable advantage of seeing the other side of the contrast exemplified: for many of them, as our Inductive Tables show us, have gone on according to the most rigorous conditions of gradual and successive generalization; and in consequence of this circumstance in their constitution, possess, in each part of their structure, a solid truth, which is always ready to stand the severest tests of reasoning and experiment.

We see how justly and clearly Bacon judged concerning the mode in which facts are to be employed in the construction of science. This, indeed, has ever been deemed his great merit: insomuch that many persons appear to apprehend the main substance of his doctrine to reside in the maxim that facts of observation, and such facts alone, are the essential elements of all true science.

10. (IV.) Yet we have endeavoured to establish the doctrine that facts are but one of two ingredients of knowledge both equally necessary;—that *Ideas* are no less indispensable than facts themselves; and that except these be duly unfolded and applied, facts are collected in vain. Has Bacon then neglected this great portion of

his subject? Has he been led by some partiality of view, or some peculiarity of circumstances, to leave this curious and essential element of science in its pristine obscurity? Was he unaware of its interest and importance?

We may reply that Bacon's philosophy; in its effect upon his readers in general, does *not* give due weight or due attention to the ideal element of our knowledge. He is considered as peculiarly and eminently the asserter of the value of experiment and observation. He is always understood to belong to the experiential, as opposed to the ideal school. He is held up in contrast to Plato and others who love to dwell upon that part of knowledge which has its origin in the intellect of man.

11. Nor can it be denied that Bacon has, in the finished part of his *Novum Organum*, put prominently forwards the necessary dependence of all our knowledge upon Experience, and said little of its dependence, equally necessary, upon the Conceptions which the intellect itself supplies. It will appear, however, on a close examination, that he was by no means insensible or careless of this internal element of all connected speculation. He held the balance, with no partial or feeble hand, between phenomena and ideas. He urged the Colligation of Facts, but he was not the less aware of the value of the Explication of Conceptions.

12. This appears plainly from some remarkable Aphorisms in the *Novum Organum*. Thus, in noticing the causes of the little progress then made by science, he states this:—"In the current Notions, all is unsound, whether they be logical or physical. *Substance, quality, action, passion, even being*, are not good Conceptions; still less are *heavy, light, dense, rare, moist, dry, generation, corruption, attraction, repulsion, element, matter, form*, and others of that kind; all are fantastical and ill-defined."



And in his attempt to exemplify his own system, he hesitates\* in accepting or rejecting the notions of *elementary*, *celestial*, *rare*, as belonging to fire, since, as he says, they are vague and ill-defined notions (*notiones vagæ nec bene terminatæ*). In that part of his work which appears to be completed, there is not, so far as I have noticed, any attempt to fix and define any notions thus complained of as loose and obscure. But yet such an undertaking appears to have formed part of his plan; and in the *Abece-darium Naturæ* †, which consists of the heads of various portions of his great scheme, marked by letters of the alphabet, we find the titles of a series of dissertations “On the Conditions of Beings,” which must have had for their object the elucidation of divers Notions essential to science, and which would have been contributions to the Explication of Conceptions, such as we have attempted in a former part of this work. Thus some of the subjects of these dissertations are;—Of Much and Little;—Of Durable and Transitory;—Of Natural and Monstrous;—Of Natural and Artificial. When the philosopher of induction came to discuss these, considered as *conditions of existence*, he could not do other than develope, limit, methodize, and define the Ideas involved in these Notions, so as to make them consistent with themselves, and a fit basis of demonstrative reasoning. His task would have been of the same nature as ours has been, in that part of this work which treats of the Fundamental Ideas of the various classes of sciences.

13. Thus Bacon, in his speculative philosophy, took firmly hold of both the handles of science; and if he had completed his scheme, would probably have given due attention to Ideas, no less than to Facts, as an element of our knowledge; while in his view of the general

\* *Nov. Org.*, Lib. II. Aph. 19.

† *Inst. Mag.*, Par. III. (Vol. VIII. p. 244.)

method of ascending from facts to principles, he displayed a sagacity truly wonderful. But we cannot be surprized, that in attempting to exemplify the method which he recommended, he should have failed. For the method could be exemplified only by some important discovery in physical science; and great discoveries, even with the most perfect methods, do not come at command. Moreover although the general structure of his scheme was correct, the precise import of some of its details could hardly be understood, till the actual progress of science had made men somewhat familiar with the kind of steps which it included.

14. (V.) Accordingly, Bacon's *Inquisition into the Nature of Heat*, which is given in the Second Book of the *Novum Organon* as an example of the mode of interrogating Nature, cannot be looked upon otherwise than as a complete failure. This will be evident if we consider that, although the exact nature of heat is still an obscure and controverted matter, the science of Heat now consists of many important truths; and that to none of these truths is there any approximation in Bacon's essay. From his process he arrives at this, as the "forma or true definition" of heat;—"that it is an expansive, restrained motion, modified in certain ways, and exerted in the smaller particles of the body." But the steps by which the science of Heat really advanced were, (as may be seen in the history\* of the subject,) these;—The discovery of a *measure* of heat or temperature (the thermometer); The establishment of the *laws* of conduction and radiation; of the *laws* of specific heat, latent heat, and the like. Such steps have led to Ampère's *hypothesis*†, that heat consists in the vibrations of an imponderable fluid; and to Laplace's *hypothesis*, that temperature consists in the internal radiation of such a fluid. These hypotheses

\* *Hist. Ind. Sci.*, B. x. c. i.

† *Ib.*, c. iv.

cannot yet be said to be even probable ; but at least they are so modified as to include some of the preceding laws which are firmly established ; whereas Bacon's hypothetical motion includes no laws of phenomena, explains no process, and is indeed itself an example of illicit generalization.

15. One main ground of Bacon's ill fortune in this undertaking appears to be, that he was not aware of an important maxim of inductive science, that we must first obtain the *measure* and ascertain the *laws* of phenomena, before we endeavour to discover their *causes*. The whole history of thermotics up to the present time has been occupied with the *former* step, and the task is not yet completed : it is no wonder, therefore, that Bacon failed entirely, when he so prematurely attempted the *second*. His sagacity had taught him that the progress of science must be gradual ; but it had not led him to judge adequately how gradual it must be, nor of what different kinds of inquiries, taken in due order, it must needs consist, in order to obtain success.

Another mistake, which could not fail to render it unlikely that Bacon should really exemplify his precepts by any actual advance in science, was, that he did not justly appreciate the sagacity, the inventive genius, which all discovery requires. He conceived that he could supersede the necessity of such peculiar endowments. "Our method of discovery in science," he says\*, "is of such a nature, that there is not much left to acuteness and strength of genius, but all degrees of genius and intellect are brought nearly to the same level." And he illustrates this by comparing his method to a pair of compasses, by means of which a person with no manual skill may draw a perfect circle. In the same spirit he speaks of proceeding by *due rejections* ; and appears to

\* *Nov. Org.*, Lib. i. Aph. 61..

imagine that when we have obtained a collection of facts, if we go on successively rejecting what is false, we shall at last find that we have, left in our hands, that scientific truth which we seek. I need not observe how far this view is removed from the real state of the case. The necessity of a *conception* which must be furnished by the mind in order to bind together the facts, could hardly have escaped the eye of Bacon, if he had cultivated more carefully the ideal side of his own philosophy. And any attempts which he could have made to construct such conceptions by mere rule and method, must have ended in convincing him that nothing but a peculiar inventive talent could supply that which was thus not contained in the facts, and yet was needed for the discovery.

16. (VI.) Since Bacon, with all his acuteness, had not divined circumstances so important in the formation of science, it is not wonderful that his attempt to reduce this process to a *Technical Form* is of little value. In the first place, he says\*, we must prepare a natural and experimental history, good and sufficient; in the next place, the instances thus collected are to be arranged in Tables in some orderly way; and then we must apply a legitimate and true induction. And in his example†, he first collects a great number of cases in which heat appears under various circumstances, which he calls “a Muster of Instances before the intellect,” (*comparentia instantiarum ad intellectum*,) or a *Table of the Presence* of the thing sought. He then adds a *Table of its Absence* in proximate cases, containing instances where heat does not appear; then a *Table of Degrees*, in which it appears with greater or less intensity. He then adds‡, that we must try to exclude several obvious suppositions, which he does by reference to some of the instances he

\* *Nov. Org.*, Lib. II. Aph. 10.

† Aph. 11.

‡ Aph. 15. p. 105.

has collected ; and this step he calls the *Exclusive*, or the *Rejection of Natures*. He then observes, (and justly,) that whereas truth emerges more easily from error than from confusion, we may, after this preparation, *give play to the intellect*, (fiat permissio intellectus,) and make an attempt at induction, liable afterwards to be corrected ; and by this step, which he terms his *First Vindemiation*, or *Inchoate Induction*, he is led to the proposition concerning heat, which we have stated above.

17. In all the details of his example he is unfortunate. By proposing to himself to examine at once into the *nature* of heat, instead of the laws of special classes of phenomena, he makes, as we have said, a fundamental mistake ; which is the less surprising since he had before him so few examples of the right course in the previous history of science. But further, his collection of instances is very loosely brought together ; for he includes in his list the *hot* taste of aromatic plants, the *caustic* effects of acids, and many other facts which cannot be ascribed to heat without a studious laxity in the use of the word. And when he comes to that point where he permits his intellect its range, the conception of *motion* upon which it at once fastens, appears to be selected with little choice or skill, the suggestion being taken from flame\*, boiling liquids, a blown fire, and some other cases. If from such examples we could imagine heat to be motion, we ought at least to have some gradation to cases of heat where no motion is visible, as in a red-hot iron. It would seem that, after a large collection of instances had been looked at, the intellect, even in its first attempts, ought not to have dwelt upon such an hypothesis as this.

18. After these steps, Bacon speaks of several classes of instances which, singling them out of the general and

\* Page 110.

indiscriminate collection of facts, he terms *Instances with Prerogative*; and these he points out as peculiar aids and guides to the intellect in its task. These Instances with Prerogative have generally been much dwelt upon by those who have commented on the *Novum Organon*. Yet, in reality, such a classification, as has been observed by one of the ablest writers of the present day\*, is of little service in the task of induction. For the instances are, for the most part, classed, not according to the ideas which they involve, or to any obvious circumstance in the facts of which they consist, but according to the extent or manner of their influence upon the inquiry in which they are employed. Thus we have Solitary Instances, Migrating Instances, Ostensive Instances, Clandestine Instances, so termed according to the degree in which they exhibit, or seem to exhibit, the property whose nature we would examine. We have Guide-Post Instances, (*Instantiæ Crucis*,) Instances of the Parted Road, of the Doorway, of the Lamp, according to the guidance they supply to our advance. Such a classification is much of the same nature as if, having to teach the art of building, we were to describe tools with reference to the amount and place of the work which they must do, instead of pointing out their construction and use:—as if we were to inform the pupil that we must have tools for lifting a stone up, tools for moving it sideways, tools for laying it square, tools for cementing it firmly. Such an enumeration of ends would convey little instruction as to the means. Moreover, many of Bacon's classes of instances are vitiated by the assumption that the "form," that is, the general law and cause of the property which is the subject of investigation, is to be looked for directly in the instances;

\* Herschel, *On the Study of Nat. Phil.*, Art. 192.

which, as we have seen in his inquiry concerning heat, is a fundamental error.

19. Yet his phraseology in some cases, as in the *instantia crucis*, serves well to mark the place which certain experiments hold in our reasonings: and many of the special examples which he gives are full of acuteness and sagacity. Thus he suggests swinging a pendulum in a mine, in order to determine whether the attraction of the earth arises from the attraction of its parts; and observing the tide at the same moment in different parts of the world, in order to ascertain whether the motion of the water is expansive or progressive; with other ingenious proposals. These marks of genius may serve to counterbalance the unfavourable judgment of Bacon's aptitude for physical science which we are sometimes tempted to form, in consequence of his false views on other points; as his rejection of the Copernican system, and his undervaluing Gilbert's magnetical speculations. Most of these errors arose from a too ambitious habit of intellect, which would not be contented with any except very wide and general truths; and from an indistinctness of mechanical, and perhaps, in general, of mathematical ideas:—defects which Bacon's own philosophy was directed to remedy, and which, in the progress of time, it has remedied in others.

20. (VII.) Having thus freely given our judgment concerning the most exact and definite portion of Bacon's precepts, it cannot be necessary for us to discuss at any length the value of those more vague and general *Warnings* against prejudice and partiality, against intellectual indolence and presumption, with which his works abound. His advice and exhortations of this kind are always expressed with energy and point, often clothed in the happiest forms of imagery; and hence it has come to pass, that such passages are perhaps more familiar to the

general reader than any other parts of his writings. Nor are Bacon's counsels without their importance, when we have to do with those subjects in which prejudice and partiality exercise their peculiar sway. Questions of politics and morals, of manners, taste, or history, cannot be subjected to a scheme of rigorous induction; and though on such matters we venture to assert general principles, these are commonly obtained with some degree of insecurity, and depend upon special habits of thought, not upon mere logical connexion. Here, therefore, the intellect may be perverted, by mixing, with the pure reason, our gregarious affections, or our individual propensities; the false suggestions involved in language, or the imposing delusions of received theories. In these dim and complex labyrinths of human thought, *the Idol of the Tribe*, or *of the Den*, or *of the Forum*, or *of the Theatre*, may occupy men's minds with delusive shapes, and may obscure or pervert their vision of truth. But in that Natural Philosophy with which we are here concerned, there is little opportunity for such influences. As far as a physical theory is completed through all the steps of a just induction, there is a clear daylight diffused over it which leaves no lurking-place for prejudice. Each part can be examined separately and repeatedly; and the theory is not to be deemed perfect till it will bear the scrutiny of all sound minds alike. Although, therefore, Bacon, by warning men against the idols or fallacious images above spoken of, may have guarded them from dangerous error, his precepts have little to do with Natural Philosophy: and we cannot agree with him when he says\*, that the doctrine concerning these idols bears the same relation to the interpretation of nature as the doctrine concerning sophistical paralogisms bears to common logic.

\* *Nov. Org.*, Lib. i. Aph. 40.



21. (VIII.) There is one very prominent feature in Bacon's speculations which we must not omit to notice; it is a leading and constant object with him to apply his knowledge to *Use*. The insight which he obtains into nature, he would employ in commanding nature for the service of man. He wishes to have not only principles but works. The phrase which best describes the aim of his philosophy is his own\*, "Ascendendo ad *axiomata*, descendendo ad *opera*." This disposition appears in the first aphorism of the *Novum Organon*, and runs through the work. "Man, the *minister* and interpreter of nature, *does* and understands, so far as he has, in fact or in thought, observed the course of nature; and he cannot know or *do* more than this." It is not necessary for us to dwell much upon this turn of mind; for the whole of our present inquiry goes upon the supposition that an acquaintance with the laws of nature is worth our having for its own sake. It may be universally true, that Knowledge is Power; but we have to do with it not as Power, but as Knowledge. It is the formation of Science, not of Art, with which we are here concerned. It may give a peculiar interest to the history of science, to show how it constantly tends to provide better and better for the wants and comforts of the body; but *that* is not the interest which engages us in our present inquiry into the nature and course of philosophy. The consideration of the means which promote man's material well-being often appears to be invested with a kind of dignity, by the discovery of general laws which it involves; and the satisfaction which rises in our minds at the contemplation of such cases, men sometimes ascribe, with a false ingenuity, to the love of mere bodily enjoyment. But it is never difficult to see that this baser and coarser element is not the real source of our admiration. Those

\* *Nov. Org.*, Lib. i. Ax. 103.

who hold that it is the main business of science to construct instruments for the uses of life, appear sometimes to be willing to accept the consequence which follows from such a doctrine, that the first shoemaker was a philosopher worthy of the highest admiration\*. But those who maintain such paradoxes, often, by a happy inconsistency, make it their own aim, not to devise some improved covering for the feet, but to delight the mind with acute speculations, exhibited in all the graces of wit and fancy.

It has been said† that the key of the Baconian doctrine consists in two words, Utility and Progress. With regard to the latter point, we have already seen that the hope and prospect of a boundless progress in human knowledge had sprung up in men's minds, even in the early times of imperial Rome; and were most emphatically expressed by that very Seneca who disdained to reckon the worth of knowledge by its value in food and clothing. And when we say that Utility was the great business of Bacon's philosophy, we forget one-half of his characteristic phrase. "Ascendendo ad axiomata," no less than "descendendo ad opera," was, he repeatedly declared, the scheme of his path. He constantly spoke, we are told by his secretary‡, of two kinds of experiments, *experimenta fructifera*, and *experimenta lucifera*.

Again; when we are told by modern writers that Bacon merely recommended such induction as all men instinctively practise, we ought to recollect his own earnest and incessant declarations to the contrary. The induction hitherto practised is, he says, of no use for obtaining solid science. There are two ways§, "*hæc via in usu est*," "*altera vera, sed intentata*." Men have con-

\* *Edinb. Rev.*, No. cxxxii. p. 65.

† *Ib.*

‡ Pref. to the *Nat. Hist.*, i. 243.

§ *Nov. Org.*, Lib. i. Aph. 19.

stantly been employed in *anticipation*; in illicit induction. The intellect left to itself rushes on in this road\*; the conclusions so obtained are persuasive†; far more persuasive than inductions made with due caution‡. But still this method must be rejected if we would obtain true knowledge. We shall then at length have ground of good hope for science when we proceed in another manner§. We must rise, not by a leap, but by small steps, by successive advances, by a gradation of ascents, trying our facts, and clearing our notions at every interval. The scheme of true philosophy, according to Bacon, is not obvious and simple, but long and technical, requiring constant care and self-denial to follow it. And we have seen that, in this opinion, his judgment is confirmed by the past history and present condition of science.

Again; it is by no means a just view of Bacon's character to place him in contrast to Plato. Plato's philosophy was the philosophy of Ideas; but it was not left for Bacon to set up the philosophy of Facts in opposition to that of Ideas. That had been done fully by the speculative reformers of the sixteenth century. Bacon had the merit of showing that Facts and Ideas must be combined; and not only so, but of divining many of the special rules and forms of this combination, when as yet there were no examples of them, with a sagacity hitherto quite unparalleled.

22. (IX.) With Bacon's unhappy political life we have here nothing to do. But we cannot but notice with pleasure how faithfully, how perseveringly, how energetically he discharged his great philosophical office of a Reformer of Methods. He had conceived the pur-

\* *Nov. Org.*, Lib. i. Aph. 20.      † Aph. 27.      ‡ *Ib.*, 28.

§ Aph. 104. So Aph. 105. "In constituendo axiomatica forma inductionis alia quam adhuc in usu fuit excogitanda est," &c.

pose of making this his object at an early period. When meditating the continuation of his *Novum Organon*, and speaking of his reasons for trusting that his work will reach some completeness of effect, he says\*, "I am by two arguments thus persuaded. First, I think thus from the zeal and constancy of my mind, which has not waxed old in this design, nor, after so many years, grown cold and indifferent; I remember that about forty years ago I composed a juvenile work about these things, which with great contrivance and a pompous title I called *temporis partum maximum*, or the most considerable birth of time; Next, that on account of its usefulness, it may hope the Divine blessing." In stating the grounds of hope for future progress in the sciences, he says†: "Some hope may, we conceive, be ministered to men by our own example: and this we say, not for the sake of boasting, but because it is useful to be said. If any despond, let them look at me, a man among all others of my age most occupied with civil affairs, nor of very sound health, (which brings a great loss of time;) also in this attempt the first explorer, following the footsteps of no man, nor communicating on these subjects with any mortal; yet, having steadily entered upon the true road and made my mind submit to things themselves, one who has, in this undertaking, made, (as we think,) some progress." He then proceeds to speak of what may be done by the combined and more prosperous labours of others, in that strain of noble hope and confidence, which rises again and again, like a chorus, at intervals in every part of his writings. In the *Advancement of Learning* he had said, "I could not be true and constant to the argument I handle, if I were not willing to go beyond others, but yet not more willing than to have others go beyond me again." In the Preface to the

\* *Ep. ad P. Fulgentium. Op.*, x. 330.    † *Nov. Org.*, i. Aph. 113.

*Instauratio Magna*, he had placed among his postulates those expressions which have more than once warmed the breast of a philosophical reformer\*. “Concerning ourselves we speak not; but as touching the matter which we have in hand, this we ask;—that men be of good hope, neither feign and imagine to themselves this our Reform as something of infinite dimension and beyond the grasp of mortal man, when in truth it is the end and true limit of infinite error; and is by no means unmindful of the condition of mortality and humanity, not confiding that such a thing can be carried to its perfect close in the space of a single age, but assigning it as a task to a succession of generations.” In a later portion of the *Instauratio* he says: “We bear the strongest love to the *human republic*, our common country; and we by no means abandon the hope that there will arise and come forth some man among posterity, who will be able to receive and digest all that is best in what we deliver; and whose care it will be to cultivate and perfect such things. Therefore, by the blessing of the Deity, to tend to this object, to open up the fountains, to discover the useful, to gather guidance for the way, shall be our task; and from this we shall never, while we remain in life, desist.”

23. (X.) We may add, that the spirit of piety as well as of hope which is seen in this passage, appears to have been habitual to Bacon at all periods of his life. We find in his works several drafts of portions of his great scheme, and several of them begin with a prayer. One of these entitled, in the edition of his works, “The Student’s Prayer,” appears to me to belong probably to his early youth. Another, entitled “The Writer’s Prayer,” is inserted at the end of the Preface of the *Instauratio*, as it was finally published. I will conclude my notice of this wonderful man by inserting here these two prayers.

\* See the motto to Kant’s *Kritik der Reinen Vernunft*.

“To God the Father, God the Word, God the Spirit, we pour forth most humble and hearty supplications; that he, remembering the calamities of mankind, and the pilgrimage of this our life, in which we wear out days few and evil, would please to open to us new refreshments out of the fountains of his goodness for the alleviating of our miseries. This also we humbly and earnestly beg, that human things may not prejudice such as are divine; neither that, from the unlocking of the gates of sense, and the kindling of a greater natural light, anything of incredulity, or intellectual night, may arise in our minds towards divine mysteries. But rather, that by our mind thoroughly cleansed and purged from fancy and vanities, and yet subject and perfectly given up to the Divine oracles, there may be given unto faith the things that are faith’s.”

“Thou, O Father, who gavest the visible light as the first-born of thy creatures, and didst pour into man the intellectual light as the top and consummation of thy workmanship, be pleased to protect and govern this work, which coming from thy goodness, returneth to thy glory. Thou, after thou hadst reviewed the works which thy hands had made, beheldest that everything was very good, and thou didst rest with complacency in them. But man, reflecting on the works which he had made, saw that all was vanity and vexation of spirit, and could by no means acquiesce in them. Wherefore, if we labour in thy works with the sweat of our brows, thou wilt make us partakers of thy vision and thy Sabbath. We humbly beg that this mind may be steadfastly in us; and that thou, by our hands, and also by the hands of others on whom thou shalt bestow the same spirit, wilt please to convey a largess of new alms to thy family of mankind. These things we commend to thy everlasting love, by our Jesus, thy Christ, God with us. Amen.”

## CHAPTER XII.

## FROM BACON TO NEWTON.

I. *Harvey*.—WE have already seen that Bacon was by no means the first mover or principal author of the revolution in the method of philosophizing which took place in his time; but only the writer who proclaimed in the most impressive and comprehensive manner, the scheme, the profit, the dignity, and the prospects of the new philosophy. Those, therefore, who after him, took up the same views are not to be considered as his successors, but as his fellow-labourers; and the line of historical succession of opinions must be pursued without special reference to any one leading character, as the principal figure of the epoch. I resume this line, by noticing a contemporary and fellow-countryman of Bacon, Harvey, the discoverer of the circulation of the blood. This discovery was not published and generally accepted till near the end of Bacon's life; but the anatomist's reflections on the method of pursuing science, though strongly marked with the character of the revolution that was taking place, belong to a very different school from the Chancellor's. Harvey was a pupil of Fabricius of Acquapendente, whom we noticed among the practical reformers of the sixteenth century. He entertained, like his master, a strong reverence for the great names which had ruled in philosophy up to that time, Aristotle and Galen; and was disposed rather to recommend his own method by exhibiting it as the true interpretation of ancient wisdom, than to boast of its novelty. It is true, that he assigns, as his reason for publishing some of his researches\*, "that by revealing the method I use in searching into things, I might pro-

\* *Anatomical Exercitationes concerning the Generation of Living Creatures*, 1653. Preface.

pose to studious men, a new and (if I mistake not) a surer path to the attainment of knowledge\* ;” but he soon proceeds to fortify himself with the authority of Aristotle. In doing this, however, he has the very great merit of giving a living and practical character to truths which exist in the Aristotelian works, but which had hitherto been barren and empty professions. We have seen that Aristotle had asserted the importance of experience as one root of knowledge ; and in this had been followed by the schoolmen of the middle ages : but this assertion came with very different force and effect from a man, the whole of whose life had been spent in obtaining, by means of experience, knowledge which no man had possessed before. In Harvey’s general reflections, the necessity of both the elements of knowledge, sensations and ideas, experience and reason, is fully brought into view, and rightly connected with the metaphysics of Aristotle. He puts the antithesis of these two elements with great clearness. “Universals are chiefly

\* He used similar expressions in conversation. George Ent, who edited his *Generation of Animals*, visited him, “at that time residing not far from the city ; and found him very intent upon the perscrutation of nature’s works, and with a countenance as cheerful, as mind unperturbed ; Democritus like, chiefly searching into the cause of natural things.” In the course of conversation the writer said, “It hath always been your choice, about the secrets of Nature, to consult Nature herself.” “Tis true,” replied he ; “and I have constantly been of opinion that from thence we might acquire not only the knowledge of those less considerable secrets of Nature, but even a certain admiration of that Supreme Essence, the Creator. And though I have ever been ready to acknowledge, that many things have been discovered by learned men of former times ; yet do I still believe that the number of those which remain yet concealed in the darkness of impervestigable Nature is much greater. Nay, I cannot forbear to wonder, and sometimes smile at those, who persuade themselves, that all things were so consummately and absolutely delivered by Aristotle, Galen, or some other great name, as that nothing was left to the superaddition of any that succeeded.”



known to us, for science is begot by reasoning from universals to particulars; yet that very comprehension of universals in the understanding springs from the perception of singulars in our sense." Again, he quotes Aristotle's apparently opposite assertions:—that made in his *Physics*\*, "that we must advance from things which are first known to us, though confusedly, to things more distinctly intelligible in themselves; from the whole to the part; from the universal to the particular;" and that made in the *Analytics*†; that "Singulars are more known to us and do first exist according to sense: for nothing is in the understanding which was not before in the sense." Both, he says, are true, though at first they seem to clash: for "though in knowledge we begin with sense, sensation itself is a universal thing." This he further illustrates; and quotes Seneca, who says, that "Art itself is nothing but the *reason* of the work, implanted in the Artist's mind:" and adds, "the same way by which we gain an Art, by the very same way we attain any kind of science or knowledge whatever; for as Art is a habit whose object is something to be done, so Science is a habit whose object is something to be known; and as the former proceedeth from the imitation of examples, so this latter, from the knowledge of things natural. The source of both is from sense and experience; since [but] it is impossible that Art should be rightly purchased by the one or Science by the other without a direction from ideas." Without here dwelling on the relation of Art and Science, (very justly stated by Harvey, except that ideas exist in a very different form in the mind of the Artist and the Scientist) it will be seen that this doctrine, of science springing from experience with a direction from ideas, is exactly that which we have repeatedly urged, as the true view of the

\* Lib. i. c. 2, 3.

+ *Anal. Post.*, II.

subject. From this view, Harvey proceeds to infer the importance of a reference to sense in his own subject, not only for first discovering, but for receiving knowledge: "Without experience, not other men's but our own, no man is a proper disciple of any part of natural knowledge; without experimental skill in anatomy, he will no better apprehend what I shall deliver concerning generation, than a man born blind can judge of the nature and difference of colours, or one born deaf, of sounds." "If we do otherwise, we may get a humid and floating opinion, but never a solid and infallible knowledge: as is happenable to those who see foreign countries only in maps, and the bowels of men falsely described in anatomical tables. And hence it comes about, that in this rank age, we have many sophisters and bookwrights, but few wise men and philosophers." He had before declared "how unsafe and degenerate a thing it is, to be tutored by other men's commentaries, without making trial of the things themselves; especially since Nature's book is so open and legible." We are here reminded of Galileo's condemnation of the "paper philosophers." The train of thought thus expressed by the practical discoverers, spread rapidly with the spread of the new knowledge that had suggested it, and soon became general and unquestioned.

II. *Descartes*.—Such opinions are now among the most familiar and popular of those which are current among writers and speakers; but we should err much if we were to imagine that after they were once propounded they were never resisted or contradicted. Indeed, even in our own time, not only are such maxims very frequently practically neglected or forgotten, but the opposite opinions, and views of science quite inconsistent with those we have been explaining, are often promulgated and widely accepted. The philosophy of pure ideas has

its commonplaces, as well as the philosophy of experience. And at the time of which we speak, the former philosophy, no less than the latter, had its great asserter and expounder; a man in his own time more admired than Bacon, regarded with more deference by a large body of disciples all over Europe, and more powerful in stirring up men's minds to a new activity of inquiry. I speak of Descartes, whose labours, considered as a philosophical system, were an endeavour to revive the method of obtaining knowledge by reasoning from our own ideas only, and to erect it in opposition to the method of observation and experiment. The Cartesian philosophy contained an attempt at a counter-revolution. Thus in this author's *Principia Philosophiæ*\*, he says that "he will give a short account of the principal phenomena of the world, not that he may use them as reasons to prove anything; for," adds he, "we desire to deduce effects from causes, not causes from effects; but only in order that out of the innumerable effects which we learn to be capable of resulting from the same causes, we may determine our mind to consider some rather than others." He had before said, "The principles which we have obtained [by pure *à priori* reasoning] are so vast and so fruitful, that many more consequences follow from them than we see contained in this visible world, and even many more than our mind can ever take a full survey of." And he professes to apply this method in detail. Thus in attempting to state the three fundamental laws of motion, he employs only *à priori* reasonings, and is in fact led into error in the third law which he thus obtains†. And in his *Dioptrics*‡ he pretends to deduce the laws of reflection and refraction of light from certain comparisons (which are, in truth, arbitrary,) in which the radiation of light is represented by

\* Pars III. p. 45. † See *Hist. Ind. Sci.*, B. VI. c. ii. ‡ Cap. I. II.

the motion of a ball impinging upon the reflecting or refracting body. It might be represented as a curious instance of the caprice of fortune, which appears in scientific as in other history, that Kepler, professing to derive all his knowledge from experience, and exerting himself with the greatest energy and perseverance, failed in detecting the law of refraction; while Descartes, who professed to be able to despise experiment, obtained the true law of sines. But as we have stated in the *History*\*, Descartes appears to have learnt this law from Snell's papers. And whether this be so or not, it is certain that notwithstanding the profession of independence which his philosophy made, it was in reality constantly guided and instructed by experience. Thus in explaining the Rainbow (in which his portion of the discovery merits great praise) he speaks† of taking a globe of glass, allowing the sun to shine on one side of it, and noting the colours produced by rays after two refractions and one reflection. And in many other instances, indeed in all that relates to physics, the reasonings and explanations of Descartes and his followers were, consciously or unconsciously, directed by the known facts, which they had observed themselves or learnt from others.

But since Descartes thus, speculatively at least, set himself in opposition to the great reform of scientific method which was going on in his time, how, it may be asked, did he acquire so strong an influence over the most active minds of his time? How is it that he became the founder of a large and distinguished school of philosophers? How is it that he not only was mainly instrumental in deposing Aristotle from his intellectual throne, but for a time appeared to have established himself with almost equal powers, and to have rendered the

\* *Hist. Ind. Sci.*, B. ix. c. ii.      † *Meteorum*, c. viii. p. 187.

Cartesian school as firm a body as the Peripatetic had been?

The causes to be assigned for this remarkable result are, I conceive, the following. In the first place, the physicists of the Cartesian school did, as I have just stated, found their philosophy upon experiment; and did not practically, nor indeed, most of them, theoretically, assent to their master's boast of showing what the phenomena *must be*, instead of looking to see what they *are*. And as Descartes had really incorporated in his philosophy all the chief physical discoveries of his own and preceding times, and had delivered, in a more general and systematic shape than any one before him, the principles which he thus established, the physical philosophy of his school was in reality far the best then current; and was an immense improvement upon the Aristotelian doctrines, which had not yet been displaced as a system. Another circumstance which gained him much favour, was the bold and ostentatious manner in which he professed to begin his philosophy by liberating himself from all preconceived prejudice. The first sentence of his philosophy contains this celebrated declaration: "Since," he says, "we begin life as infants, and have contracted various judgments concerning sensible things before we possess the entire use of our reason, we are turned aside from the knowledge of truth by many prejudices: from which it does not appear that we can be any otherwise delivered, than if once in our life we make it our business to doubt of everything in which we discern the smallest suspicion of uncertainty." In the face of this sweeping rejection or unhesitating scrutiny of all preconceived opinions, the power of the ancient authorities and masters in philosophy must obviously shrink away; and thus Descartes came to be considered as the great hero of the overthrow of the Aristotelian dogmatism. But in

addition to these causes, and perhaps more powerful than all, in procuring the assent of men to his doctrines, came the deductive and systematic character of his philosophy. For although all knowledge of the external world is in reality only to be obtained from observation, by inductive steps,—minute, perhaps, and slow, and many, as Galileo and Bacon had already taught;—the human mind conforms to these conditions reluctantly and unsteadily, and is ever ready to rush to general principles, and then to employ itself in deducing conclusions from these by synthetical reasonings; a task grateful, from the distinctness and certainty of the result, and the accompanying feeling of our own sufficiency. Hence men readily overlooked the precarious character of Descartes' fundamental assumptions, in their admiration of the skill with which a varied and complex Universe was evolved out of them. And the complete and systematic character of this philosophy attracted men no less than its logical connexion. I may quote here what a philosopher\* of our own time has said of another writer: "He owed his influence to various causes; at the head of which may be placed that genius for system which, though it cramps the growth of knowledge, perhaps finally atones for that mischief by the zeal and activity which it rouses among followers and opponents, who discover truth by accident when in pursuit of weapons for their warfare. A system which attempts a task so hard as that of subjecting vast provinces of human knowledge to one or two principles, if it presents some striking instances of conformity to superficial appearances, is sure to delight the framer; and for a time to subdue and captivate the student too entirely for sober reflection and rigorous examination. In the first instance consistency passes for truth. When principles in

\* Mackintosh, *Dissertation on Ethical Science*.

some instances have proved sufficient to give an unexpected explanation of facts, the delighted reader is content to accept as true all other deductions from the principles. Specious premises being assumed to be true, nothing more can be required than logical inference. Mathematical forms pass current as the equivalent of mathematical certainty. The unwary admirer is satisfied with the completeness and symmetry of the plan of his house, unmindful of the need of examining the firmness of the foundation and the soundness of the materials. The system-maker, like the conqueror, long dazzles and overawes the world; but when their sway is past, the vulgar herd, unable to measure their astonishing faculties, take revenge by trampling on fallen greatness." Bacon had showed his wisdom in his reflections on this subject, when he said that "Method, carrying a show of total and perfect knowledge, hath a tendency to generate acquiescence."

The main value of Descartes' physical doctrines consisted in their being arrived at in a way inconsistent with his own professed method, namely, by a reference to observation. But though he did in reality begin from facts, his system was nevertheless a glaring example of that error which Bacon had called *Anticipation*; that illicit generalization which leaps at once from special facts to principles of the widest and remotest kind; such, for instance, as the Cartesian doctrine, that the world is an absolute *plenum*, every part being full of matter of some kind, and that all natural effects depend on the laws of motion. Against this fault, to which the human mind is so prone, Bacon had lifted his warning voice in vain, so far as the Cartesians were concerned; as indeed, to this day, one theorist after another pursues his course, and turns a deaf ear to the Verulamian injunctions; perhaps even complacently boasts that he founds his theory

upon observation; and forgets that there are, as the aphorism of the *Novum Organon* declares, two ways by which this may be done;—the one hitherto in use and suggested by our common tendencies, but barren and worthless; the other almost untried, to be pursued only with effort and self-denial, but alone capable of producing true knowledge.

III. *Gassendi*.—Thus the lessons which Bacon taught were far from being generally accepted and applied at first. The amount of the influence of these two men, Bacon and Descartes, upon their age, has often been a subject of discussion. The fortunes of the Cartesian school have been in some measure traced in the History of Science. But I may mention the notice taken of these two philosophers by Gassendi, a contemporary and countryman of Descartes. Gassendi, as I have elsewhere stated\*, was associated with Descartes in public opinion, as an opponent of the Aristotelian dogmatism; but was not in fact a follower or profound admirer of that writer. In a Treatise on Logic, Gassendi gives an account of the Logic of various sects and authors; treating, in order, of the Logic of Zeno (the Eleatic), of Euclid (the Megarean), of Plato, of Aristotle, of the Stoics, of Epicurus, of Lullius, of Ramus; and to these he adds the Logic of Verulam, and the Logic of Cartesius. “We must not,” he says, “on account of the celebrity it has obtained, pass over the *Organon* or Logic of Francis Bacon Lord Verulam, High Chancellor of England, whose noble purpose in our time it has been, to make an Instauration of the Sciences.” He then gives a brief account of the *Novum Organon*, noticing the principal features in its rules, and especially the distinction between the vulgar induction which leaps at once from particular experiments to the more general axioms, and the chastised and gradual in-

\* *Hist. Ind. Sci.*, B. VII. c. i.



duction, which the author of the *Organon* recommends. In his account of the Cartesian Logic, he justly observes, that "He too imitated Verulam in this, that being about to build up a new philosophy from the foundation, he wished in the first place to lay aside all prejudice: and having then found some solid principle, to make that the ground-work of his whole structure. But he proceeds by a very different path from that which Verulam follows; for while Verulam seeks aid from things, to perfect the cogitation of the intellect, Cartesius conceives, that when we have laid aside all knowledge of things, there is, in our thoughts alone, such a resource, that the intellect may by its own power arrive at a perfect knowledge of all, even the most abstruse things."

The writings of Descartes have been most admired, and his method most commended, by those authors who have employed themselves upon metaphysical rather than physical subjects of inquiry. Perhaps we might say that, in reference to such subjects, this method is not so vicious as at first, when contrasted with the Baconian induction, it seems to be: for it might be urged that the *thoughts* from which Descartes begins his reasonings are, in reality *experiments* of the kind which the subject requires us to consider: each such thought is a fact in the intellectual world; and of such facts, the metaphysician seeks to discover the laws. I shall not here examine the validity of this plea; but shall turn to the consideration of the actual progress of physical science and its effect on men's minds.

IV. *Actual progress in Science.*—The practical discoverers were indeed very active and very successful during the seventeenth century which opened with Bacon's survey and exhortations. The laws of nature, of which men had begun to obtain a glimpse in the preceding century, were investigated with zeal and saga-

city, and the consequence was that the foundations of most of the modern physical sciences were laid. That mode of research by experiment and observation, which had, a little time ago, been a strange, and to many, an unwelcome innovation, was now become the habitual course of philosophers. The revolution from the philosophy of tradition to the philosophy of experience was completed. The great discoveries of Kepler belonged to the preceding century. They are not, I believe, noticed, either by Bacon or by Descartes; but they gave a strong impulse to astronomical and mechanical speculators, by showing the necessity of a sound science of motion. Such a science Galileo had already begun to construct. At the time of which I speak, his disciples\* were still labouring at this task, and at other problems which rapidly suggested themselves. They had already convinced themselves that air had weight; in 1643 Torricelli proved this practically by the invention of the Barometer; in 1647, Pascal proved it still further by sending the Barometer to the top of a mountain. Pascal and Boyle brought into clear view the fundamental laws of fluid equilibrium; Boyle and Mariotte determined the law of the compression of air as regulated by its elasticity. Otto Guericke invented the air pump, and by his "Magdeburg Experiments" on a vacuum, illustrated still further the effects of the air. Guericke pursued what Gilbert had begun, the observation of electrical phenomena; and these two physicists made an important step, by detecting repulsion as well as attraction in these phenomena. Gilbert had already laid the foundations of the science of Magnetism. The law of refraction, at which Kepler had laboured in vain, was, as we have seen, discovered by Snell (about 1621), and published by Descartes.

\* Castelli, Torricelli, Viviani, Baliani, Gassendi, Mersenne, Borelli, Cavalleri.

Mersenne had discovered some of the more important parts of the theory of Harmonics. In sciences of a different kind, the same movement was visible. Chemical doctrines tended to assume a proper degree of generality, when Sylvius in 1679 taught the opposition of acid and alkali, and Stahl, soon after, the phlogistic theory of combustion. Steno had remarked the most important law of crystallography in 1669, that the angles of the same kind of crystals are always equal. In the sciences of classification, about 1680, Ray and Morison in England resumed the attempt to form a systematic botany, which had been interrupted for a hundred years, from the time of the memorable essay of Cæsalpinus. The grand discovery of the circulation of the blood by Harvey about 1619, was followed in 1651 by Pecquet's discovery of the course of the chyle. There could now no longer be any question whether science was progressive, or whether observation could lead to new truths.

Among these cultivators of science, such sentiments as have been already quoted became very familiar ;—that knowledge is to be sought from nature herself by observation and experiment ;—that in such matters tradition is of no force when opposed to experience, and that mere reasonings without facts cannot lead to solid knowledge. But I do not know that we find in these writers any more special rules of induction and scientific research which have since been confirmed and universally adopted. Perhaps too, as was natural in so great a revolution, the writers of this time, especially the second-rate ones, were somewhat too prone to disparage the labours and talents of Aristotle and the ancients in general, and to overlook the ideal element of our knowledge, in their zealous study of phenomena. They urged, sometimes in an exaggerated manner, the superiority of modern times in all that regards science, and the supreme and sole importance of facts in

scientific investigations. There prevailed among them also a lofty and dignified tone of speaking of the condition and prospects of science, such as we are accustomed to admire in the Verulamian writings; for this, in a less degree, is epidemic among those who a little after his time speak of the new philosophy.

V. *Otto Guericke, &c.*—I need not illustrate these characteristics at any great length. I may as an example notice Otto Guericke's Preface to his *Experimenta Magdeburgica* (1670). He quotes a passage from Kircher's Treatise on the Magnetic Art, in which the author says, "Hence it appears how all philosophy, except it be supported by experiments, is empty, fallacious, and useless; what monstrosities philosophers, in other respects of the highest and subtlest genius, may produce in philosophy by neglecting experiment. Thus Experience alone is the Dissolver of Doubts, the Reconciler of Difficulties, the sole Mistress of Truth, who holds a torch before us in obscurity, unties our knots, teaches us the true causes of things." Guericke himself reiterates the same remark, adding that "philosophers, insisting upon their own thoughts and arguments merely, cannot come to any sound conclusion respecting the natural constitution of the world." Nor were the Cartesians slow in taking up the same train of reflection. Thus Gilbert Clark who, in 1660, published\* a defence of Descartes' doctrine of a *plenum* in the universe, speaks in a tone which reminds us of Bacon, and indeed was very probably caught from him. "Natural philosophy formerly consisted entirely of loose and most doubtful controversies, carried on in high sounding words, fit rather to delude than to instruct men. But at last (by the favour of the Deity) there shone forth some more divine intellects, who taking as their counsel-

\* *De Plenitudine Mundi, in qua defenditur Cartesiana Philosophia contra sententias Francisci Baconi, Th. Hobbi et Sethi Wardi.*

lors reason and experience together, exhibited a new method of philosophizing. Hence has been conceived a strong hope that philosophers may embrace, not a shadow or empty image of Truth, but Truth herself: and that Physiology (Physics) scattering these controversies to the winds, will contract an alliance with Mathematics. Yet this is hardly the work of one age; still less of one man. Yet let not the mind despond, or doubt not that, one party of investigators after another following the same method of philosophizing, at last, under good auguries, the mysteries of nature being daily unlocked as far as human feebleness will allow, Truth may at last appear in full, and these nuptial torches may be lighted."

As another instance of the same kind, I may quote the Preface to the First volume of the Transactions of the Academy of Sciences at Paris. "It is only since the present century," says the writer, "that we can reckon the revival of Mathematics and Physics. M. Descartes and other great men have laboured at this work with so much success, that in this department of literature, the whole face of things has been changed. Men have quitted a sterile system of physics, which for several generations had been always at the same point; the reign of words and terms is passed; men will have things; they establish principles which they understand, they follow those principles; and thus they make progress. Authority has ceased to have more weight than Reason: that which was received without contradiction because it had been long received, is now examined, and often rejected: and philosophers have made it their business to consult, respecting natural things, Nature herself rather than the Ancients." These had now become the commonplaces of those who spoke concerning the course and method of the Sciences.

VI. *Hooke*.—In England, as might be expected, the

influence of Francis Bacon was more directly visible. We find many writers, about this time, repeating the truths which Bacon had proclaimed, and in almost every case showing the same imperfections in their views which we have noticed in him. We may take as an example of this Hooke's Essay, entitled "A General Scheme or Idea of the present state of Natural Philosophy, and how its defects may be remedied by a Methodical proceeding in the making Experiments and collecting Observations; whereby to compile a Natural History as a solid basis for the superstructure of true Philosophy." This Essay may be looked upon as an attempt to adapt the *Novum Organon* to the age which succeeded its publication. We have in this imitation, as in the original, an enumeration of various mistakes and impediments which had in preceding times prevented the progress of knowledge; exhortations to experiment and observation as the only solid basis of Science; very ingenious suggestions of trains of inquiry, and modes of pursuing them; and a promise of obtaining scientific truths when facts have been duly accumulated. This last part of his scheme the author calls a *Philosophical Algebra*; and he appears to have imagined that it might answer the purpose of finding unknown causes from known facts, by means of certain regular processes, in the same manner as Common Algebra finds unknown from known quantities. But this part of the plan appears to have remained unexecuted. The suggestion of such a method was a result of the Baconian notion that invention in a discoverer might be dispensed with. We find Hooke adopting the phrases in which this notion is implied: thus he speaks of the understanding as "being very prone to run into the affirmative way of judging, and wanting patience to follow and prosecute the negative way of inquiry, by rejection of disagreeing natures." And he follows Bacon also in the error of

attempting at once to obtain from the facts the discovery of a "nature," instead of investigating first the measures and the laws of phenomena. I return to more general notices of the course of men's thoughts on this subject.'

VII. *Royal Society*.—Those who associated themselves together for the prosecution of science quoted Bacon as their leader, and exulted in the progress made by the philosophy which proceeded upon his principles. Thus in Oldenburg's Dedication of the Transactions of the Royal Society of London for 1670, to Robert Boyle, he says; "I am informed by such as well remember the best and worst days of the famous Lord Bacon, that though he wrote his *Advancement of Learning* and his *Instauratio Magna* in the time of his greatest power, yet his greatest reputation rebounded first from the most intelligent foreigners in many parts of Christendom:" and after speaking of his practical talents and his public employments, he adds, "much more justly still may we wonder how, without any great skill in Chemistry, without much pretence to the Mathematics or Mechanics, without optic aids or other engines of late invention, he should so much transcend the philosophers then living, in judicious and clear instructions, in so many useful observations and discoveries, I think I may say beyond the records of many ages." And in the end of the Preface to the same volume, he speaks with great exultation of the advance of science all over Europe, referring undoubtedly to facts then familiar. "And now let envy snarl, it cannot stop the wheels of active philosophy, in no part of the known world;—not in France, either in Paris or in Caen:—not in Italy, either in Rome, Naples, Milan, Florence, Venice, Bononia or Padua;—in none of the Universities either on this or on that side of the seas, Madrid and Lisbon, all the best spirits in Spain and Portugal, and the spacious and remote dominions to them

belonging ;—the Imperial Court and the Princes of Germany ; the Northern Kings and their best luminaries ; and even the frozen Muscovite and Russian have all taken the operative ferment : and it works high and prevails every way, to the encouragement of all sincere lovers of knowledge and virtue.”

Again, in the Preface for 1672, he pursues the same thought into detail. “ We must grant that in the last age, when operative philosophy began to recover ground, and to tread on the heels of triumphant Philology ; emergent adventures and great successes were encountered by dangerous oppositions and strong obstructions. Galilæus and others in Italy suffered extremities for their celestial discoveries ; and here in England Sir Walter Raleigh, when he was in his greatest lustre, was notoriously slandered to have erected a school of atheism, because he gave countenance to chemistry, to practical arts, and to curious mechanical operations, and designed to form the best of them into a college. And Queen Elizabeth’s Gilbert was a long time esteemed extravagant for his magnetisms ; and Harvey for his diligent researches in pursuance of the circulation of the blood. But when our renowned Lord Bacon had demonstrated the methods for a perfect restoration of all parts of real knowledge ; and the generous and philosophical Peireskious had, soon after, agitated in all parts to redeem the most instructive antiquities, and to excite experimental essays and fresh discoveries ; the success became on a sudden stupendous ; and effective philosophy began to sparkle, and even to flow into beams of shining light all over the world.”

The formation of the Royal Society of London and of the Academy of Sciences of Paris, from which proceeded the declamations just quoted, were among many indications, belonging to this period, of the importance



which states as well as individuals had by this time begun to attach to the cultivation of science. The English Society was established almost immediately when the restoration of the monarchy appeared to give a promise of tranquillity to the nation (in 1660), and the French Academy very soon afterwards (in 1666). These measures were very soon followed by the establishment of the Observatories of Paris and Greenwich (in 1667 and 1675); which may be considered to be a kind of public recognition of the astronomy of observation, as an object on which it was the advantage and the duty of nations to bestow their wealth.

VIII. *Bacon's New Atalantis*.—When philosophers had their attention turned to the boundless prospect of increase to the knowledge and powers and pleasures of man which the cultivation of experimental philosophy seemed to promise, it was natural that they should think of devising institutions and associations by which such benefits might be secured. Bacon had drawn a picture of a society organized with a view to such purpose, in his fiction of the “New Atalantis.” The imaginary teacher who explains this institution to the inquiring traveller, describes it by the name of *Solomon's House*; and says\*, “The end of our foundation is the knowledge of causes and secret motions of things; and the enlarging the bounds of the human empire to effecting of things possible.” And, as parts of this House, he describes caves and wells, chambers and towers, baths and gardens, parks and pools, dispensatories and furnaces, and many other contrivances, provided for the purpose of making experiments of many kinds. He describes also the various employments of the Fellows of this College, who take a share in its researches. There are *merchants of light*, who bring books and inventions from foreign countries;

\* Bacon's *Works*, Vol. II. 111.

*depredators*, who gather the experiments which exist in books; *mystery-men*, who collect the experiments of the mechanical arts; *pioneers* or *miners*, who invent new experiments; and *compilers*, “who draw the experiments of the former into titles and tables, to give the better light for the drawing of observations and axioms out of them.” There are also *dowry-men* or *benefactors*, that cast about how to draw out of the experiments of their fellows things of use and practice for man’s life; *lamps*, that direct new experiments of a more penetrating light than the former; *inoculators*, that execute the experiments so directed. Finally, there are the *interpreters of nature*, that raise the former discoveries by experiments into greater observations (that is, more general truths) axioms and aphorisms. Upon this scheme we may remark, that fictitious as it undisguisedly is, it still serves to exhibit very clearly some of the main features of the author’s philosophy:—namely, his steady view of the necessity of ascending from facts to the most general truths by several stages;—an exaggerated opinion of the aid that could be derived in such a task from technical separation of the phenomena and a distribution of them into tables;—a belief, probably incorrect, that the offices of experimenter and interpreter may be entirely separated, and pursued by different persons with a certainty of obtaining success;—and a strong determination to make knowledge constantly subservient to the uses of life.

IX. *Cowley*.—Another project of the same kind, less ambitious but apparently more directed to practice, was published a little later (1657) by another eminent man of letters in this country. I speak of Cowley’s “Proposition for the Advancement of Experimental Philosophy.” He suggests that a College should be established at a short distance from London, endowed with a revenue

of four thousand pounds, and consisting of twenty professors with other members. The objects of the labours of these professors he describes to be, first, to examine all knowledge of nature delivered to us from former ages and to pronounce it sound or worthless; second, to recover the lost inventions of the ancients; third, to improve all arts that we now have; lastly, to discover others that we yet have not. In this proposal we cannot help marking the visible declension from Bacon's more philosophical view. For we have here only a very vague indication of improving old arts and discovering new, instead of the two clear Verulamian antitheses, Experiments and Axioms deduced from them, on the one hand, and on the other an ascent to general Laws, and a derivation, from these, of Arts for daily use. Moreover the prominent place which Cowley has assigned to the verifying the knowledge of former ages and recovering "the lost inventions and drowned lands of the ancients," implies a disposition to think too highly of traditionary knowledge; a weakness which Bacon's scheme shows *him* to have fully overcome. And thus it has been up to the present day, that with all Bacon's mistakes, in the philosophy of scientific method few have come up to him, and perhaps none have gone beyond him.

Cowley exerted himself to do justice to the new philosophy in verse as well as prose, and his Poem to the Royal Society expresses in a very noble manner those views of the history and prospects of philosophy which prevailed among the men by whom the Royal Society was founded. The fertility and ingenuity of comparison which characterize Cowley's poetry are well known; and these qualities are in this instance largely employed for the embellishment of his subject. Many of the comparisons which he exhibits are apt and striking. Philosophy is a ward whose estate (human knowledge) is, in

his nonage, kept from him by his guardians and tutors ; (a case which the ancient rhetoricians were fond of taking as a subject of declamation ; ) and these wrong-doers retain him in unjust tutelage and constraint for their own purposes ; until

Bacon at last, a mighty man, arose,  
(Whom a wise King, and Nature, chose  
Lord Chancellor of both their laws,)  
And boldly undertook the injured pupil's cause.

Again, Bacon is one who breaks a scarecrow Priapus which stands in the garden of knowledge. Again, Bacon is one who, instead of a picture of painted grapes, gives us real grapes from which we press "the thirsty soul's refreshing wine." Again, Bacon is like Moses, who led the Hebrews forth from the barren wilderness, and ascended Pisgah ;—

Did on the very border stand  
Of the blest promised land,  
And from the mountain's top of his exalted wit  
Saw it himself and showed us it.

The poet however adds, that Bacon discovered, but did not conquer this new world ; and that the men whom he addresses must subdue these regions. These "champions" are then ingeniously compared to Gideon's band :

Their old and empty pitchers first they brake  
And with their hands then lifted up the light.

There were still at this time some who sneered at or condemned the new philosophy ; but the tide of popular opinion was soon strongly in its favour. I have elsewhere\* noticed a pasquinade of the poet Boileau in 1682, directed against the Aristotelians. At this time, and indeed for long afterwards, the philosophers of France were Cartesians. The English men of science,

\* *Hist. Ind. Sci.*, B. vii. c. i.

although partially and for a time they accepted some of Descartes' opinions, for the most part carried on the reform independently, and in pursuance of their own views. And they very soon found a much greater leader than Descartes to place at their head, and to take as their authority, so far as they acknowledged authority, in their speculations. I speak of Newton, whose influence upon the philosophy of science I must now consider.

### CHAPTER XIII.

#### NEWTON.

1. BOLD and extensive as had been the anticipations of those whose minds were excited by the promise of the new philosophy, the discoveries of Newton respecting the mechanics of the universe, brought into view truths more general and profound than those earlier philosophers had hoped or imagined. With these vast accessions to human knowledge, men's thoughts were again set in action; and philosophers made earnest and various attempts to draw, from these extraordinary advances in science, the true moral with regard to the conduct and limits of the human understanding. They not only endeavoured to verify and illustrate, by these new portions of science, what had recently been taught concerning the methods of obtaining sound knowledge; but they were also led to speculate concerning many new and more interesting questions relating to this subject. They saw, for the first time, or at least far more clearly than before, the distinction between the inquiry into the *laws*, and into the *causes* of phenomena. They were tempted to ask, how far the discovery of causes could be carried;

and whether it would soon reach, or clearly point to, the ultimate cause. They were driven to consider whether the properties which they discovered were essential properties of all matter, necessarily and primarily involved in its essence, though revealed to us at a late period by their derivative effects. These questions even now agitate the thoughts of speculative men. Some of them have already, in this work, been discussed, or arranged in the places which our view of the philosophy of these subjects assigns to them. But we must here notice them as they occurred to Newton himself and his immediate followers.

2. The general Baconian notion of the method of philosophizing, that it consists in ascending from phenomena, through various stages of generalization, to truths of the highest order, received, in Newton's discovery of the universal mutual gravitation of every particle of matter, that pointed actual exemplification, for want of which it had hitherto been almost overlooked, or at least very vaguely understood. That great truth, and the steps by which it was established, afford, even now, by far the best example of the successive ascent, from one scientific truth to another,—of the repeated transition from less to more general propositions,—which we can yet produce; as may be seen in the Table which exhibits the relation of these steps in Book XI. Newton himself did not fail to recognize this feature in the truths which he exhibited. Thus, he says\*, “By the way of Analysis we proceed from compounds to ingredients, as from motions to the forces producing them; and in general, from effects to their causes, and from particular causes to more general ones, till the argument end in the most general.” And in like manner in another Query†: “The main business of natural philo-

\* *Opticks*, Qu. 31, near the end.

† Qu. 28.

sophy is to argue from phenomena without feigning hypotheses, and to deduce causes from effects, till we come to the First Cause, which is certainly not mechanical."

3. Newton appears to have had a horror of the term *hypothesis*, which probably arose from his acquaintance with the rash and illicit general assumptions of Descartes. Thus in the passage just quoted, after declaring that gravity must have some other cause than matter, he says, "Later philosophers banish the consideration of such a cause out of Natural Philosophy, feigning hypotheses for explaining all things mechanically, and referring other causes to metaphysics." In the celebrated Scholium at the end of the *Principia*, he says, "Whatever is not deduced from the phenomena, is to be termed *hypothesis*; and hypotheses, whether metaphysical or physical, or occult causes, or mechanical, have no place in experimental philosophy. In this philosophy, propositions are deduced from phenomena, and rendered general by induction." And in another place, he arrests the course of his own suggestions, saying, "Verum hypotheses non fingo." I have already attempted to show that this is, in reality, a superstitious and self-destructive spirit of speculation. Some hypotheses are necessary, in order to connect the facts which are observed; some new principle of unity must be applied to the phenomena, before induction can be attempted. What is requisite is, that the hypothesis should be close to the facts, and not connected with them by other arbitrary and untried facts; and that the philosopher should be ready to resign it as soon as the facts refuse to confirm it. We have seen in the *History*, that it was by such a use of hypotheses, that both Newton himself, and Kepler, on whose discoveries those of Newton were based, made their discoveries. The suppositions of a force tending to the sun and varying inversely as the square of the

distance; of a mutual force between all the bodies of the solar system; of the force of each body arising from the attraction of all its parts; not to mention others, also propounded by Newton,—were all hypotheses before they were verified as theories. It is related that when Newton was asked how it was that he saw into the laws of nature so much further than other men, he replied, that if it were so, it resulted from his keeping his thoughts steadily occupied upon the subject which was to be thus penetrated. But what is this occupation of the thoughts, if it be not the process of keeping the phenomena clearly in view, and trying, one after another, all the plausible hypotheses which seem likely to connect them, till at last the true law is discovered? Hypotheses so used are a necessary element of discovery.

4. With regard to the details of the process of discovery, Newton has given us some of his views, which are well worthy of notice, on account of their coming from him; and which are real additions to the philosophy of this subject. He speaks repeatedly of the *analysis* and *synthesis* of observed facts; and thus marks certain steps in scientific research, very important, and not, I think, clearly pointed out by his predecessors. Thus he says\*, “As in Mathematics, so in Natural Philosophy, the investigation of difficult things by the method of analysis ought ever to precede the method of composition. This analysis consists in making experiments and observations, and in drawing general conclusions from them by induction, and admitting of no objections against the conclusions, but such as are taken from experiments or other certain truths. And although the arguing from experiments and observations by induction be no demonstration of general conclusions; yet it is the best way of arguing which the nature of things

\* *Opticks*, Qu. 31.



admits of, and may be looked upon as so much the stronger, by how much the induction is more general." And he then observes, as we have quoted above, that by this way of analysis we proceed from compounds to ingredients, from motions to forces, from effects to causes, and from less to more general causes. The *analysis* here spoken of includes the steps which in this work we call the *decomposition* of facts, the exact *observation* and *measurement* of the phenomena, and the *colligation* of facts; the necessary intermediate step, the *selection* and *explication* of the appropriate conception, being passed over, in the fear of seeming to encourage the fabrication of hypotheses. The *synthesis* of which Newton here speaks consists of those steps of *deductive reasoning*, proceeding from the conception once assumed, which are requisite for the comparison of its consequences with the observed facts. This statement of the process of research, is, as far as it goes, perfectly exact.

5. In speaking of Newton's precepts on the subject, we are naturally led to the celebrated "Rules of Philosophizing," inserted in the second edition of the *Principia*. These rules have generally been quoted and commented on with an almost unquestioning reverence. Such Rules, coming from such an authority, cannot fail to be highly interesting to us; but at the same time, we cannot here evade the necessity of scrutinizing their truth and value, according to the principles which our survey of this subject has brought into view. The Rules stand at the beginning of that part of the *Principia* (the Third Book) in which he infers the mutual gravitation of the sun, moon, planets, and all parts of each. They are as follows:

"Rule I. We are not to admit other causes of natural things than such as both are true, and suffice for explaining their phenomena.

“Rule II. Natural effects of the same kind are to be referred to the same causes, as far as can be done.

“Rule III. The qualities of bodies which cannot be increased or diminished in intensity, and which belong to all bodies in which we can institute experiments, are to be held for qualities of all bodies whatever.

“Rule IV. In experimental philosophy, propositions collected from phenomena by induction, are to be held as true either accurately or approximately, notwithstanding contrary hypotheses; till other phenomena occur by which they may be rendered either more accurate or liable to exception.”

In considering these Rules, we cannot help remarking, in the first place, that they are constructed with an intentional adaptation to the case with which Newton has to deal,—the induction of Universal Gravitation; and are intended to protect the reasonings before which they stand. Thus the first Rule is designed to strengthen the inference of gravitation from the celestial phenomena, by describing it as a *vera causa*, a true cause; the second countenances the doctrine that the planetary motions are governed by mechanical forces, as terrestrial motions are; the third rule appears intended to justify the assertion of gravitation, as a *universal* quality of bodies; and the fourth contains, along with a general declaration of the authority of induction, the author's usual protest against hypotheses, levelled at the Cartesian hypotheses especially.

6. *Of the First Rule.*—We, however, must consider these Rules in their general application, in which point of view they have often been referred to, and have had very great authority allowed them. One of the points which has been most discussed, is that maxim which requires that the causes of phenomena which we assign should be true causes, *veræ causæ*. Of course this does

not mean that they should be *the* true or right cause; for although it is the philosopher's aim to discover such causes, he would be little aided in his search of truth, by being told that it is truth which he is to seek. The rule has generally been understood to prescribe that in attempting to account for any class of phenomena, we must assume such causes only, as *from other considerations*, we know to exist. Thus gravity, which was employed in explaining the motions of the moon and planets, was already known to exist and operate at the earth's surface.

Now the Rule thus interpreted is, I conceive, an injurious limitation of the field of induction. For it forbids us to look for a cause, except among the causes with which we are already familiar. But if we follow this rule, how shall we ever become acquainted with any new cause? Or how do we know that the phenomena which we contemplate do really arise from some cause which we already truly know? If they do not, must we still insist upon making them depend upon some of our known causes; or must we abandon the study of them altogether? Must we, for example, resolve to refer the action of radiant heat to the air, rather than to any peculiar fluid or ether, because the former is known to exist, the latter is merely assumed for the purpose of explanation? But why should we do this? Why should we not endeavour to learn the cause from the effects, even if it be not already known to us? We can infer causes, which are new when we first become acquainted with them. Chemical Forces, Optical Forces, Vital Forces, are known to us only by chemical and optical and vital phenomena; must we, therefore, reject their existence or abandon their study? They do not conform to the double condition, that they shall be sufficient and *also* real: they are true, only so far as they explain the

facts, but are they, therefore, unintelligible or useless? Are they not highly important and instructive subjects of speculation? And if the gravitation which rules the motions of the planets had not existed at the earth's surface;—if it had been there masked and concealed by the superior effect of magnetism, or some other extraneous force, might not Newton still have inferred, from Kepler's laws, the tendency of the planets to the sun; and from their perturbations, their tendency to each other? His discoveries would still have been immense, if the cause which he assigned had not been a *vera causa* in the sense now contemplated.

7. But what do we mean by calling gravity a “true cause?” How do we learn its reality? Of course, by its effects, with which we are familiar;—by the weight and fall of bodies about us. These strike even the most careless observer. No one can fail to see that all bodies which we come in contact with are heavy;—that gravity acts in our neighbourhood here upon earth. Hence, it may be said, this cause is at any rate a true cause, whether it explains the celestial phenomena or not.

But if this be what is meant by a *vera causa*, it appears strange to require that in all cases we should find such a one to account for all classes of phenomena. Is it reasonable or prudent to demand that we shall reduce every set of phenomena, however minute, or abstruse, or complicated, to causes so obviously existing as to strike the most incurious, and to be familiar among men? How can we expect to find *such veræ causæ* for the delicate and recondite phenomena which an exact and skilful observer detects in chemical, or optical, or electrical experiments? The facts themselves are too fine for vulgar apprehension; their relations, their symmetries, their measures require a previous discipline to understand them. How then can their causes be found

among those agencies with which the common unscientific herd of mankind are familiar? What likelihood is there that causes held for real by such persons, shall explain facts which such persons cannot see or cannot understand?

Again: if we give authority to such a rule, and require that the causes by which science explains the facts which she notes and measures and analyzes, shall be causes which men, without any special study, have already come to believe in, from the effects which they casually see around them, what is this, except to make our first rude and unscientific persuasions the criterion and test of our most laborious and thoughtful inferences? What is it, but to give to ignorance and thoughtlessness the right of pronouncing upon the convictions of intense study and long-disciplined thought? "Electrical atmospheres" surrounding electrized bodies, were at one time held to be a "true cause" of the effects which such bodies produce. These atmospheres, it was said, are obvious to the senses; we feel them like a spider's web on the hands and face. *Æpinus* had to answer such persons, by proving that there are no atmospheres, no effluvia, but only repulsion. He thus, for a *true cause* in the vulgar sense of the term, substituted an *hypothesis*; yet who doubts that what he did was an advance in the science of electricity?

8. Perhaps some persons may be disposed to say, that Newton's Rule does not enjoin us to take those causes only which we clearly know, or suppose we know, to be really existing and operating, but only causes *of such kinds* as we have already satisfied ourselves do exist in nature. It may be urged that we are entitled to infer that the planets are governed in their motions by an attractive force, because we find, in the bodies immediately subject to observation and experiment, that such

motions are produced by attractive forces, for example by that of the earth. It may be said that we might on similar grounds infer forces which unite particles of chemical compounds, or deflect particles of light, because we see adhesion and deflection produced by forces.

But it is easy to show that the Rule, thus laxly understood, loses all significance. It prohibits no hypothesis; for all hypotheses suppose causes *such as*, in some case or other, we have seen in action. No one would think of explaining phenomena by referring them to forces and agencies altogether different from any which are known; for on this supposition, how could he pretend to reason about the effects of the assumed causes, or undertake to prove that they would explain the facts? Some close similarity with some known kind of cause is requisite, in order that the hypothesis may have the appearance of an explanation. No forces, or virtues, or sympathies, or fluids, or ethers, would be excluded by *this* interpretation of *veræ causæ*. Least of all, would such an interpretation reject the Cartesian hypothesis of vortices; which undoubtedly, as I conceive, Newton intended to condemn by his Rule. For that *such* a case as a whirling fluid, carrying bodies round a center in orbits, does occur, is too obvious to require proof. Every eddying stream, or blast that twirls the dust in the road, exhibits examples of such action, and would justify the assumption of the vortices which carry the planets in their courses; as indeed, without doubt, such facts suggested the Cartesian explanation of the solar system. The vortices, in this mode of considering the subject, are at the least as *real* a cause of motion as gravity itself.

9. Thus the Rule which enjoins "true causes," is nugatory, if we take *veræ causæ* in the extended sense of any causes of a real *kind*, and unphilosophical if we

understand the term of *those very* causes which we familiarly suppose to exist. But it may be said that we are to designate as "true causes," not those which are collected in a loose, confused and precarious manner, by undisciplined minds, from obvious phenomena, but those which are justly and rigorously inferred. Such a cause, it may be added, gravity is; for the facts of the downward pressures and downward motions of bodies at the earth's surface lead us, by the plainest and strictest induction, to the assertion of such a force. Now to this interpretation of the Rule there is no objection; but then, it must be observed, that on this view, terrestrial gravity is inferred by the same process as celestial gravitation; and the cause is no more entitled to be called "true," because it is obtained from the former, than because it is obtained from the latter class of facts. We thus obtain an intelligible and tenable explanation of a *vera causa*; but then, by this explanation, its *verity* ceases to be distinguishable from its other condition, that it "suffices for the explanation of the phenomena." The assumption of universal gravitation accounts for the fall of a stone; it also accounts for the revolutions of the Moon or of Saturn; but since both these explanations are of the same kind, we cannot with justice make the one a criterion or condition of the admissibility of the other.

10. But still, the Rule, so understood, is so far from being unmeaning or frivolous, that it expresses one of the most important tests which can be given of a sound physical theory. It is true, the explanation of one set of facts may be of the same nature as the explanation of the other class: but then, that the cause explains *both* classes, gives it a very different claim upon our attention and assent from that which it would have if it explained one class only. The very circumstance that the two

explanations coincide, is a most weighty presumption in their favour. It is the testimony of two witnesses in behalf of the hypothesis; and in proportion as these two witnesses are separate and independent, the conviction produced by their agreement is more and more complete. When the explanation of two kinds of phenomena, distinct, and not apparently connected, leads us to the same cause, such a coincidence does give a reality to the cause, which it has not while it merely accounts for those appearances which suggested the supposition. This coincidence of propositions inferred from separate classes of facts, is exactly what we noticed in the last Book, as one of the most decisive characteristics of a true theory, under the name of *Consilience of Inductions*.

That Newton's First Rule of Philosophizing, so understood, authorizes the inferences which he himself made, is really the ground on which they are so firmly believed by philosophers. Thus when the doctrine of a gravity varying inversely as the square of the distance from the body, accounted at the same time for the relations of times and distances in the planetary orbits and for the amount of the moon's deflection from the tangent of her orbit, such a doctrine became most convincing: or again, when the doctrine of the universal gravitation of all parts of matter, which explained so admirably the inequalities of the moon's motions, also gave a satisfactory account of a phenomenon utterly different, the precession of the equinoxes. And of the same kind is the evidence in favour of the undulatory theory of light, when the assumption of the length of an undulation, to which we are led by the colours of thin plates, is found to be identical with that length which explains the phenomena of diffraction; or when the hypothesis of transverse vibrations, suggested by the facts of polarization, explains also the laws of double refraction. When such



a convergence of two trains of induction points to the same spot, we can no longer suspect that we are wrong. Such an accumulation of proof really persuades us that we have to do with a *vera causa*. And if this kind of proof be multiplied;—if we again find other facts of a sort un contemplated in framing our hypothesis, but yet clearly accounted for when we have adopted the supposition;—we are still further confirmed in our belief; and by such accumulation of proof we may be so far satisfied, as to believe without conceiving it possible to doubt. In this case, when the validity of the opinion adopted by us has been repeatedly confirmed by its sufficiency in unforeseen cases, so that all doubt is removed and forgotten, the theoretical cause takes its place among the realities of the world, and becomes a *true cause*.

11. Newton's Rule then, to avoid mistakes, might be thus expressed; That "we may, provisorily, assume such hypothetical cause as will account for any given class of natural phenomena; but that when two different classes of facts lead us to the same hypothesis, we may hold it to be a *true cause*." And this Rule will rarely or never mislead us. There are no instances, in which a doctrine recommended in this manner has afterwards been discovered to be false. There have been hypotheses which have explained many phenomena, and kept their ground long, and have afterwards been rejected. But these have been hypotheses which explained only one class of phenomena; and their fall took place when another kind of facts was examined and brought into conflict with the former. Thus the system of eccentrics and epicycles accounted for all the observed *motions* of the planets, and was the means of expressing and transmitting all astronomical knowledge for two thousand years. But then, how was it overthrown? By considering the *dis-*

*tances* as well as motions of the heavenly bodies. Here was a second class of facts; and when the system was adjusted so as to agree with the one class, it was at variance with the other. These cycles and epicycles could not be true, because they could not be made a just representation of the facts. But if the measures of distance as well as of position had conspired in pointing out the cycles and epicycles, as the paths of the planets, the paths so determined could not have been otherwise than their real paths; and the epicyclical theory would have been, at least geometrically, true.

12. *Of the Second Rule.*—Newton's Second Rule directs that "natural events of the *same kind* are to be referred to the *same causes*, so far as can be done." Such a precept at first appears to help us but little; for all systems, however little solid, profess to conform to such a rule. When any theorist undertakes to explain a class of facts, he assigns causes which, according to him, will by their natural action, as seen in other cases, produce the effects in question. The events which he accounts for by his hypothetical cause, are, he holds, of the same kind as those which such a cause is known to produce. Kepler, in ascribing the planetary motions to magnetism, Descartes, in explaining them by means of vortices, held that they were referring celestial motions to the causes which give rise to terrestrial motions of the same kind. The question is, *Are* the effects of the same kind? This once settled, there will be no question about the propriety of assigning them to the same cause. But the difficulty is, to determine *when* events are of the same kind. Are the motions of the planets of the same kind with the motion of a body moving freely in a curvilinear path, or do they not rather resemble the motion of a floating body swept round by a whirling current? The Newtonian and the Cartesian answered this question

differently. How then can we apply this Rule with any advantage?

13. To this we reply, that there is no way of escaping this uncertainty and ambiguity, but by obtaining a clear possession of the ideas which our hypothesis involves, and by reasoning rigorously from them. Newton asserts that the planets move in free paths, acted on by certain forces. The most exact calculation gives the closest agreement of the results of this hypothesis with the facts. Descartes asserts that the planets are carried round by a fluid. The more rigorously the conceptions of force and the laws of motion are applied to this hypothesis, the more signal is its failure in reconciling the facts to one another. Without such calculation, we can come to no decision between the two hypotheses. If the Newtonian hold that the motions of the planets are *evidently* of the *same kind* as those of a body describing a curve in free space, and therefore, like that, to be explained by a force acting upon the body; the Cartesian denies that the planets do move in free space. They are, he maintains, immersed in a plenum. It is only when it appears that comets pass through this plenum in all directions with no impediment, and that no possible form and motion of its whirlpools can explain the forces and motions which are observed in the solar system, that he is compelled to allow the Newtonian's classification of events of the *same kind*.

Thus it does not appear that this Rule of Newton can be interpreted in any distinct and positive manner, otherwise than as enjoining that, in the task of induction, we employ clear ideas, rigorous reasoning, and close and fair comparison of the results of the hypothesis with the facts. These are, no doubt, important and fundamental conditions of a just induction; but in this injunction we find no peculiar or technical criterion

by which we may satisfy ourselves that we are right, or detect our errors. Still, of such general prudential rules, none can be more wise than one which thus, in the task of connecting facts by means of ideas, recommends that the ideas be clear, the facts, correct, and the chain of reasoning which connects them, without a flaw.

14. *Of the Third Rule.*—The Third Rule, that “qualities which are observed without exception be held to be universal,” as I have already said, seems to be intended to authorize the assertion of gravitation as a universal attribute of matter. We formerly stated, in treating of Mechanical Ideas\*, that this application of such a Rule appears to be a mode of reasoning far from conclusive. The assertion of the universality of any property of bodies must be grounded upon the reason of the case, and not upon any arbitrary maxim. Is it intended by this Rule to prohibit any further examination how far gravity is an original property of matter, and how far it may be resolved into the result of other agencies? We know perfectly well that this was not Newton’s intention; since the cause of gravity was a point which he proposed to himself as a subject of inquiry. It would certainly be very unphilosophical to pretend, by this Rule of Philosophizing, to prejudge the question of such hypotheses as that of Mosotti, That gravity is the excess of the electrical attraction over electrical repulsion: and yet to adopt this hypothesis, would be to suppose electrical forces more truly universal than gravity; for according to the hypothesis, gravity, being the inequality of the attraction and repulsion, is only an accidental and partial relation of these forces. Nor would it be allowable to urge this Rule as a reason of assuming that double stars are attracted to each other by a force varying according to the inverse square of the distance;

\* Book III. c. x.

without examining, as Herschel and others have done, the orbits which they really describe. But if the Rule is not available in such cases, what is its real value and authority? and in what cases are they exemplified?

15. In a former part of this work\*, it was shown that the fundamental laws of motion, and the properties of matter which these involve, are, after a full consideration of the subject, unavoidably assumed as universally true. It was further shown, that although our knowledge of these laws and properties be gathered from experience, we are strongly impelled, some philosophers think, authorized, to look upon these as not only universally, but necessarily true. It was also stated, that the law of gravitation, though its universality may be deemed probable, does not apparently involve the same necessity as the fundamental laws of motion. But it was pointed out that these are some of the most abstruse and difficult questions of the whole of philosophy; involving the profound, perhaps insoluble, problem of the identity or diversity of Ideas and Things. It cannot, therefore, be deemed philosophical to cut these Gordian knots by peremptory maxims, which encourage us to decide without rendering a reason. Moreover, it appears clear that the reason which is rendered for this Rule by the Newtonians is quite untenable; namely, that we know extension, hardness, and inertia, to be universal qualities of bodies by experience alone, and that we have the same evidence of experience for the universality of gravitation. We have already observed that we cannot, with any propriety, say that we *find* by experience all bodies are extended. This could not be a just assertion, except we could conceive the possibility of our finding the contrary. But who can conceive our finding by experience some bodies which are not extended? It appears, then, that the reason

\* Book III. c. ix. x. xi.

given for the Third Rule of Newton involves a mistake respecting the nature and authority of experience. And the Rule itself cannot be applied without attempting to decide, by the casual limits of observation, questions which necessarily depend upon the relations of ideas.

16. *Of the Fourth Rule.*—Newton's Fourth Rule is, that "Propositions collected from phenomena by induction, shall be held to be true, notwithstanding contrary hypotheses; but shall be liable to be rendered more accurate, or to have their exceptions pointed out, by additional study of phenomena." This Rule contains little more than a general assertion of the authority of induction, accompanied by Newton's usual protest against hypotheses.

The really valuable part of the Fourth Rule is that which implies that a constant verification, and, if necessary, rectification, of truths discovered by induction, should go on in the scientific world. Even when the law is, or appears to be, most certainly exact and universal, it should be constantly exhibited to us afresh in the form of experience and observation. This is necessary, in order to discover exceptions and modifications if such exist; and if the law be rigorously true, the contemplation of it, as exemplified in the world of phenomena, will best give us that clear apprehension of its bearings which may lead us to see the ground of its truth.

The concluding clause of this Fourth Rule appears, at first, to imply that all inductive propositions are to be considered as merely provisional and limited, and never secure from exception. But to judge thus would be to underrate the stability and generality of scientific truths; for what man of science can suppose that we shall hereafter discover exceptions to the universal gravitation of all parts of the solar system? And it is plain that the

author did not intend the restriction to be applied so rigorously; for in the Third Rule, as we have just seen he authorizes us to infer universal properties of matter from observation, and carries the liberty of inductive inference to its full extent. The Third Rule appears to encourage us to assert a law to be universal, even in cases in which it has not been tried; the Fourth Rule seems to warn us that the law may be inaccurate, even in cases in which it has been tried. Nor is either of these suggestions erroneous; but both the universality and the rigorous accuracy of our laws are proved by reference to Ideas rather than to Experience; a truth which, perhaps, the philosophers of Newton's time were somewhat disposed to overlook.

17. The disposition to ascribe all our knowledge to Experience, appears in Newton and the Newtonians by other indications; for instance, it is seen in their extreme dislike to the ancient expressions by which the principles and causes of phenomena were described, as the *occult causes* of the Schoolmen, and the *forms* of the Aristotelians, which had been adopted by Bacon. Newton says\*, that the particles of matter not only possess inertia, but also active principles, as gravity, fermentation, cohesion; he adds, "These principles I consider not as Occult Qualities, supposed to result from the Specific Forms of things, but as General Laws of Nature, by which the things themselves are formed: their truth appearing to us by phenomena, though their causes be not yet discovered. For these are manifest qualities, and their causes only are occult. And the Aristotelians gave the name of *occult qualities*, not to manifest qualities, but to such qualities only as they supposed to lie hid in bodies, and to the unknown causes of manifest effects: such as would be the causes of gravity, and of mag-

\* *Optics*, Qu. 31.

netick and electrick attractions, and of fermentations, if we should suppose that these forces or actions arose from qualities unknown to us, and incapable of being discovered and made manifest. Such occult qualities put a stop to the improvement of Natural Philosophy, and therefore of late years have been rejected. To tell us that every species of things is endowed with an occult specific quality by which it acts and produces manifest effects, is to tell us nothing: but to derive two or three general principles of motion from phenomena, and afterwards to tell us how the properties and actions of all corporeal things follow from these manifest principles, would be a great step in philosophy, though the causes of those principles were not yet discovered: and therefore I scruple not to propose the principles of motion above maintained, they being of very general extent, and leave their causes to be found out."

18. All that is here said is highly philosophical and valuable; but we may observe that the investigation of *specific forms*, in the sense in which some writers had used the phrase, was by no means a frivolous or unmeaning object of inquiry. Bacon and others had used *form* as equivalent to *law*\*. If we could ascertain that arrangement of the particles of a crystal from which its external crystalline form and other properties arise, this arrangement would be the *internal form* of the crystal. If the undulatory theory be true, the *form* of light is transverse vibrations: if the emission theory be maintained, the *form*

\* *Nov. Org.*, Lib. II. Aph. 2. Licet enim in natura nihil existet præter corpora individua, edentia actus puros individuos ex lege; in doctrinis tamen illa ipsa lex, ejusque inquisitio, et inventio, et explicatio, pro fundamento est tam ad sciendum quam ad operandum. Eam autem *legem*, ejusque *paragraphos*, *formarum* nomine intelligimus; præsertim cum hoc vocabulum invaluerit, et famuliter occurrat.

Aph. 17. Eadem res est *forma* calidi vel *forma* luminis, et *lex* calidi aut *lex* luminis.



of light is particles moving in straight lines, and deflected by various forces. Both the terms, *form* and *law*, imply an ideal connexion of sensible phenomena; form supposes matter which is moulded to the form; law supposes objects which are governed by the law. The former term refers more precisely to existences, the latter to occurrences. The latter term is now the more familiar, and is, perhaps, the better metaphor: but the former also contains the essential antithesis which belongs to the subject, and might be used in expressing the same conclusions.

But occult causes, employed in the way in which Newton describes, had certainly been very prejudicial to the progress of knowledge, by stopping inquiry with a mere word. The absurdity of such pretended explanations had not escaped ridicule. The pretended physician in the comedy gives an example of an occult cause or virtue.

Mihi demandatur  
A doctissimo Doctore  
*Quare* Opium facit dormire:  
Et ego respondeo,  
*Quia* est in eo  
*Virtus dormitiva*,  
Cujus natura est sensus assoupire.

19. But the most valuable part of the view presented to us in the quotation just given from Newton is the distinct separation, already noticed as peculiarly brought into prominence by him, of the determination of the *laws* of phenomena, and the investigation of their *causes*. The maxim, that the former inquiry must precede the latter, and that if the general laws of facts be discovered, the result is highly valuable, although the causes remain unknown, is extremely important; and had not, I think, ever been so strongly and clearly stated, till Newton both

repeatedly promulgated the precept, and added to it the weight of the most striking examples.

We have seen that Newton, along with views the most just and important concerning the nature and methods of science, had something of the tendency, prevalent in his time, to suspect or reject, at least speculatively, all elements of knowledge except observation. This tendency was, however, in him so corrected and restrained by his own wonderful sagacity and mathematical habits, that it scarcely led to any opinion which we might not safely adopt. But we must now consider the cases in which this tendency operated in a more unbalanced manner, and led to the assertion of doctrines which, if consistently followed, would destroy the very foundations of all general and certain knowledge.

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#### CHAPTER XIV.

##### LOCKE AND HIS FRENCH FOLLOWERS.

1. IN the constant opposition and struggle of the schools of philosophy, which consider our Senses and our Ideas, respectively, as the principal sources of our knowledge, we have seen that at the period of which we now treat, the tendency was to exalt the external and disparage the internal element. The disposition to ascribe our knowledge to observation alone, had already, in Bacon's time, led him to dwell to a disproportionate degree upon that half of his subject; and had tinged Newton's expressions, though it had not biassed his practice. But this partiality soon assumed a more prominent shape, becoming extreme in Locke, and extravagant in those who professed to follow him.

Indeed Locke appears to owe his popularity and influence as a popular writer mainly to his being one of the first to express, in a plain and unhesitating manner, opinions which had for some time been ripening in the minds of a large portion of the cultivated public. Hobbes had already promulgated the main doctrines, which Locke afterwards urged, on the subject of the origin and nature of our knowledge: but in him these doctrines were combined with offensive opinions on points of morals, government, and religion, so that their access to general favour was impeded: and it was to Locke that they were indebted for the extensive influence which they soon after obtained. Locke owed this authority mainly to the intellectual circumstances of the time. Although a writer of great merit, he by no means possesses such metaphysical acuteness or such philosophical largeness of view, or such a charm of writing, as must necessarily give him the high place he has held in the literature of Europe. But he came at a period when the reign of Ideas was tottering to its fall. All the most active and ambitious spirits had gone over to the new opinions, and were prepared to follow the fortunes of the Philosophy of Experiment, then in the most prosperous and brilliant condition, and full of still brighter promise. There were, indeed, a few learned and thoughtful men who still remained faithful to the empire of Ideas; partly, it may be, from a too fond attachment to ancient systems; but partly, also, because they knew that there were subjects of vast importance, in which experience did not form the whole foundation of our knowledge. They knew, too, that many of the plausible tenets of the new philosophy were revivals of fallacies which had been discussed and refuted in ancient times. But the advocates of mere experience came on with a vast store of weighty truth among their artillery, and with the energy which the advance usually

bestows. The ideal system of philosophy could, for the present, make no effectual resistance; Locke, by putting himself at the head of the assault, became the hero of his day: and his name has been used as the watchword of those who adhere to the philosophy of the senses up to our own times.

2. Locke himself did not assert the exclusive authority of the senses in the extreme unmitigated manner in which some who call themselves his disciples have done. But this is the common lot of the leaders of revolutions, for they are usually bound by some ties of affection and habit to the previous state of things, and would not destroy all traces of that condition: while their followers attend, not to their inconsistent wishes, but to the meaning of the revolution itself; and carry out, to their genuine and complete results, the principles which won the victory, and which have been brought out more sharp from the conflict. Thus Locke himself does not assert that all our ideas are derived from Sensation, but from Sensation *and Reflection*. But it was easily seen that, in this assertion, two very heterogeneous elements were conjoined: that while to pronounce Sensation the origin of ideas, is a clear decided tenet, the acceptance or rejection of which determines the general character of our philosophy; to make the same declaration concerning Reflection, is in the highest degree vague and ambiguous; since reflection may either be resolved into a mere modification of sensation, as was done by one school, or may mean all that the opposite school opposes to sensation, under the name of Ideas. Hence the clear and strong impression which fastened upon men's minds, and which does in fact represent all the systematic and consistent part of Locke's philosophy, was, that in it all our ideas are represented as derived from Sensation.

3. We need not spend much time in pointing out

the inconsistencies into which Locke fell; as all must fall into inconsistencies who recognize no source of knowledge except the senses. Thus he maintains that our Idea of Space is derived from the senses of sight and touch; our Idea of Solidity from the touch alone. Our Notion of Substance is an unknown support of unknown qualities, and is illustrated by the Indian fable of the tortoise which supports the elephant, which supports the world. Our Notion of Power or Cause is in like manner got from the senses. And yet, though these ideas are thus mere fragments of our experience, Locke does not hesitate to ascribe to them necessity and universality when they occur in propositions. Thus he maintains the necessary truth of geometrical properties: he asserts that the resistance arising from solidity is absolutely insurmountable\*; he conceives that nothing short of Omnipotence can annihilate a particle of matter†; and he has no misgivings in arguing upon the axiom that Every thing must have a cause. He does not perceive that, upon his own account of the origin of our knowledge, we can have no right to make any of these assertions. If our knowledge of the truths which concern the external world were wholly derived from experience, all that we could venture to say would be,—that geometrical properties of figures are true *as far as we have tried them*;—that we have seen *no example* of a solid body being reduced to occupy less space by pressure, or of a material substance annihilated by natural means;—and that *whenever we have examined*, we have found that every change has had a cause. Experience can never entitle us to declare that what she has not seen is impossible; still less, that things which she can not see are certain. Locke himself intended to throw no doubt upon the certainty of either human or divine knowledge; but his principles, when

\* Book XI. c. iv. sect. 3.

† *Ib.*, c. xiii. sect. 22.

men discarded the temper in which he applied them, and the checks to their misapplication which he conceived that he had provided, easily led to a very comprehensive scepticism. His doctrines tended to dislodge from their true bases the most indisputable parts of knowledge; as, for example, pure and mixed mathematics. It may well be supposed, therefore, that they shook the foundations of many other parts of knowledge in the minds of common thinkers.

It was not long before these consequences of the overthrow of ideas showed themselves in the speculative world. I have already in a previous part of this work\* mentioned Hume's sceptical inferences from Locke's maxim, that we have no ideas except those which we acquire by experience; and the doctrines set up in opposition to this by the metaphysicians of Germany. I might trace the progress of the sensational opinions in Britain till the reaction took place here also: but they were so much more clearly and decidedly followed out in France, that I shall pursue their history in that country.

4. *The French Followers of Locke, Condillac, &c.*—Most of the French writers who adopted Locke's leading doctrines, rejected the "Reflection," which formed an anomalous part of his philosophy, and declared that Sensation alone was the source of ideas. Among these writers, Condillac was the most distinguished. He expressed the leading tenet of their school in a clear and pointed manner by saying that "All ideas are transformed sensations." We have already considered this phrase†, and need not here longer dwell upon it.

Opinions such as these tend to annihilate, as we have seen, one of the two co-ordinate elements of our know-

\* Book III. c. iii. Modern Opinions respecting the Idea of Cause.

† B. I. c. iv.

ledge. Yet they were far from being so prejudicial to the progress of science, or even of the philosophy of science, as might have been anticipated. One reason of this was, that they were practically corrected, especially among the cultivators of Natural Philosophy, by the study of mathematics; for that study did really supply all that was requisite on the ideal side of science, so far as the ideas of space, time, and number, were concerned, and partly also with regard to the idea of cause and others. And the methods of discovery, though the philosophy of them made no material advance, were practically employed with so much activity, and in so many various subjects, that a certain kind of prudence and skill in this employment was very widely diffused.

5. *Importance of Language.*—In one respect this school of metaphysicians rendered a very valuable service to the philosophy of science. They brought into prominent notice the great importance of *words* and *terms* in the formation and progress of knowledge, and pointed out that the office of language is not only to convey and preserve our thoughts, but to perform the analysis in which reasoning consists. They were led to this train of speculation, in a great measure, by taking pure mathematical science as their standard example of substantial knowledge. Condillac, rejecting, as we have said, almost all those ideas on which universal and demonstrable truths must be based, was still not at all disposed to question the reality of human knowledge; but was, on the contrary, a zealous admirer of the evidence and connexion which appear in those sciences which have the ideas of space and number for their foundation, especially the latter. He looked for the grounds of the certainty and reality of the knowledge which these sciences contain; and found them, as he conceived, in the

nature of the *language* which they employ. The *Signs* which are used in arithmetic and algebra enable us to keep steadily in view the identity of the same quantity under all the forms which, by composition and decomposition, it may be made to assume; and these Signs also not only express the operations which are performed, but suggest the extension of the operations according to analogy. Algebra, according to him, is only a very perfect language; and language answers its purpose of leading us to truth, by possessing the characteristics of algebra. Words are the symbols of certain groups of impressions or facts; they are so selected and applied as to exhibit the analogies which prevail among these facts; and these analogies are the truths of which our knowledge consists. "Every language is an analytical method; every analytical method is a language\*;" these were the truths "alike new and simple," as he held, which he conceived that he had demonstrated. "The art of speaking, the art of writing, the art of reasoning, the art of thinking, are only, at bottom, one and the same art†." Each of these operations consists in a succession of analytical operations; and words are the marks by which we are able to fix our minds upon the steps of this analysis.

6. The analysis of our impressions and notions does in reality lead to truth, not only in virtue of the identity of the whole with its parts, as Condillac held, but also in virtue of certain Ideas which govern the synthesis of our sensations, and which contain the elements of universal truths, as we have all along endeavoured to show. But although Condillac overlooked or rejected this doctrine, the importance of words, as marking the successive steps of this synthesis and analysis, is not less than he represented it to be. Every truth, once established by induc-

\* *Langue des Calculs*, p. 1.

† *Grammaire*, p. xxxvi.



tion from facts, when it is become familiar under a brief and precise form of expression, becomes itself a fact; and is capable of being employed, along with other facts of a like kind, as the materials of fresh inductions. In this successive process, the term, like the cord of a fagot, both binds together the facts which it includes, and makes it possible to manage the assemblage as a single thing. On occasion of most discoveries in science, the selection of a technical term is an essential part of the proceeding. In the *History of Science*, we have had numerous opportunities of remarking this; and the List of technical terms given as an Index to that work, refers us, by almost every word, to one such occasion. And these terms, which thus have had so large a share in the formation of science, and which constitute its language, do also offer the means of analyzing its truths, each into *its* constituent truths; and these into facts more special, till the original foundations of our most general propositions are clearly exhibited. The relations of general and particular truths are most evidently represented by the Inductive Tables given in Book XI. But each step in each of these Tables has its proper form of expression, familiar among the cultivators of science; and the analysis which our Tables display, is commonly performed in men's minds, when it becomes necessary, by fixing the attention successively upon a series of words, not upon the lines of a Table. Language offers to the mind such a scale or ladder as the Table offers to the eye; and as such Tables present to us, as we have said, the Logic of Induction, that is, the formal conditions of the soundness of our reasoning from facts, we may with propriety say that a just analysis of the meaning of words is an essential portion of Inductive Logic.

In saying this, we must not forget that a decomposition of general truths into ideas, as well as into facts,

belongs to our philosophy; but the point we have here to remark, is the essential importance of words to the latter of these processes. And this point had not ever had its due weight assigned to it till the time of Condillac and other followers of Locke, who pursued their speculations in the spirit I have just described. The doctrine of the importance of terms is the most considerable addition to the philosophy of science which has been made since the time of Bacon\*.

7. *The French Encyclopædists*.—The French *Encyclopédie*, published in 1751, of which Diderot and Dalember were the editors, may be considered as representing the leading characters of European philosophy during the greater part of the eighteenth century. The writers in this work belong for the most part to the school of Locke and Condillac; and we may make a few remarks upon them, in order to bring into view one or two points in addition to what we have already said of that school. The *Discours Préliminaire*, written by Dalember, is celebrated as containing a view of the origin of our knowledge, and the connexion and classification of the sciences.

A tendency of the speculations of the Encyclopedists, as of the School of Locke in general, is to reject all ideal principles of connexion among facts, as something which experience, the only source of true knowledge, does not give. Hence all certain knowledge consists only in the recognition of the same thing under different aspects, or different forms of expression. Axioms are not the result

\* Since the selection and construction of terms is thus a matter of so much consequence in the formation of science, it is proper that systematic rules, founded upon sound principles, should be laid down for the performance of this operation. Some such rules are accordingly suggested in a subsequent part of this work.

of an original relation of ideas, but of the use, or it may be the abuse\*, of words. In like manner, the propositions of Geometry are a series of modifications,—of distortions, so to speak,—of one original truth; much as if the proposition were stated in the successive forms of expression presented by a language which was constantly growing more and more artificial. Several of the sciences which rest upon physical principles, that is, (says the writer) truths of experience or simple hypotheses, have only an experimental or hypothetical certainty. Impenetrability added to the idea of extent is a mystery in addition: the nature of motion is a riddle for philosophers: the metaphysical principle of the laws of percussion is equally concealed from them. The more profoundly they study the idea of matter and of the properties which represent it, the more obscure this idea becomes; the more completely does it escape them.

8. This is a very common style of reflection, even down to our own times. I have endeavoured to show that concerning the Fundamental Ideas of space, of force and resistance, of substance, external quality, and the like, we know enough to make these Ideas the grounds of certain and universal truths;—enough to supply us with axioms from which we can demonstratively reason. If men wish for any other knowledge of the nature of matter than that which ideas, and facts conformable to ideas, give them, undoubtedly their desire will be frustrated, and they will be left in a mysterious vacancy; for it does not appear how such knowledge as they ask for could be knowledge at all. But in reality, this complaint of our ignorance of the real nature of things proceeds from the rejection of ideas, and the assumption of the senses alone as the ground of knowledge. “Obser-

\* *Disc. Prélim.*, p. viii.

vation and calculation are the only sources of truth:”—this is the motto of the school of which we now speak. And its import amounts to this:—that they reject all ideas except the idea of number, and recognize the modifications which parts undergo by addition and subtraction as the only modes in which true propositions are generated. The laws of nature are assemblages of facts: the truths of science are assertions of the identity of things which are the same. “By the avowal of almost all philosophers,” says a writer of this school\*, “the most sublime truths, when once simplified and reduced to their lowest terms, are converted into facts, and thenceforth present to the mind only this proposition; the white is white, the black is black.”

These statements are true in what they positively assert, but they involve error in the denial which by implication they convey. It is true that observation and demonstration are the only sources of scientific truth; but then, demonstration may be founded on other grounds besides the elementary properties of number. It is true that the theory of gravitation is but the assertion of a general fact; but this is so, not because a sound theory does not involve ideas, but because our apprehension of a fact does.

9. Another characteristic indication of the temper of the Encyclopedists and of the age to which they belong, is the importance by them assigned to those practical *Arts* which minister to man's comfort and convenience. Not only, in the body of the Encyclopedia, are the Mechanical Arts placed side by side with the Sciences, and treated at great length; but in the Preliminary Discourse, the preference assigned to the liberal over the mechanical Arts is treated as a prejudice†, and the value of science is spoken of as measured by its

\* Helvetius *Sur l'Homme*, c. xxiii.

† P. xiii.

utility. "The discovery of the Mariner's Compass is not less advantageous to the human race than the explanation of its properties would be to physics.—Why should we not esteem those to whom we owe the fusee and the escapement of watches as much as the inventors of Algebra?" And in the classification of sciences which accompanies the Discourse, the labours of artisans of all kinds have a place.

This classification of the various branches of science contained in the Dissertation is often spoken of. It has for its basis the classification proposed by Bacon, in which the parts of human knowledge are arranged according to the faculties of the mind in which they originate; and these faculties are taken, both by Bacon and by Dalember, as Memory, Reason, and Imagination. The insufficiency of Bacon's arrangement as a scientific classification is so glaring, that the adoption of it, with only superficial modifications, at the period of the Encyclopedia, is a remarkable proof of the want of original thought and real philosophy at the time of which we speak.

10. We need not trace further the opinion which derives all our knowledge from the senses in its application to the philosophy of Science. Its declared aim is to reduce all knowledge to the knowledge of Facts; and it rejects all inquiries which involve the Idea of Cause, and similar Ideas, describing them as "metaphysical," or in some other damnable way. It professes, indeed, to discard all Ideas; but, as we have long ago seen, some Ideas or other are inevitably included even in the simplest Facts. Accordingly the speculations of this school are compelled to retain the relations of Position, Succession, Number and Resemblance, which are rigorously ideal relations. The philosophy of Sensation, in order to be consistent, ought to reject these Ideas along with

the rest, and to deny altogether the possibility of general knowledge.

When the opinions of the Sensational School had gone to an extreme length, a Reaction naturally began to take place in men's minds. Such have been the alternations of opinion, from the earliest ages of human speculation. Man may perhaps have existed in an original condition in which he was only aware of the impressions of Sense; but his first attempts to analyze his perceptions brought under his notice Ideas as a separate element, essential to the existence of knowledge. Ideas were thenceforth almost the sole subject of the study of philosophers; of Plato and his disciples, professedly; of Aristotle, and still more of the followers and commentators of Aristotle, practically. And this continued till the time of Galileo, when the authority of the Senses again began to be asserted; for it was shown by the great discoveries which were then made, that the Senses had at least some share in the promotion of knowledge. As discoveries more numerous and more striking were supplied by Observation, the world gradually passed over to the opinion that the share which had been ascribed to Ideas in the formation of real knowledge was altogether a delusion, and that Sensation alone was true. But when this was asserted as a general doctrine, both its manifest falsity and its alarming consequences roused men's minds, and made them recoil from the extreme point to which they were approaching. Philosophy again oscillated back towards Ideas; and over a great part of Europe, in the clearest and most comprehensive minds, this regression from the dogmas of the Sensational School is at present the prevailing movement. We shall conclude our review by noticing a few indications of this state of things.

## CHAPTER XV.

## THE REACTION AGAINST THE SENSATIONAL SCHOOL.

1. WHEN Locke's *Essay* appeared, it was easily seen that its tendency was to urge, in a much more rigorous sense than had previously been usual, the ancient maxim of Aristotle, adopted by the schoolmen of the middle ages, that "nothing exists in the intellect but what has entered by the senses." Leibnitz expressed in a pointed manner the limitation with which this doctrine had always been understood. "Nihil est in intellectu quod non prius fuerit in sensu;—*nempe*," he added, "*nisi intellectus ipse*." To this it has been objected\*, that we cannot say that the intellect is *in* the intellect. But this remark is obviously frivolous; for the faculties of the understanding (which are what the argument against the Sensational School requires us to reserve) may be said to be *in* the understanding, with as much justice as we may assert there are *in* it the impressions derived from sense. And when we take account of these faculties, and of the Ideas to which, by their operation, we necessarily subordinate our apprehension of phenomena, we are led to a refutation of the philosophy which makes phenomena, unconnected by Ideas, the source of all knowledge. The succeeding opponents of the Lockian school insisted upon and developed in various ways this remark of Leibnitz, or some equivalent view.

2. It was by inquiries into the foundations of Morals that English philosophers were led to question the truth of Locke's theory. Dr. Price, in his *Review of the Principal Questions in Morals*, first published in 1757, maintained that we cannot with propriety assert all our ideas

\* See Mr. Sharpe's *Essays*.

to be derived from sensation and reflection. He pointed out, very steadily, the other source. "The power, I assert, that *understands*, or the faculty within us that discerns *truth*, and that compares all the objects of thought and *judges* of them, is a spring of new ideas\*." And he exhibits the antithesis in various forms. "Were not *sense* and *knowledge* entirely different, we should rest satisfied with sensible impressions, such as light, colours and sounds, and inquire no further about them, at least when the impressions are strong and vigorous: whereas, on the contrary, we necessarily desire some further acquaintance with them, and can never be satisfied till we have subjected them to the survey of reason. Sense presents *particular* forms to the mind, but cannot rise to any *general* ideas. It is the intellect that examines and compares the presented forms, that rises above individuals to universal and abstract ideas; and thus looks downward upon objects, takes in at one view an infinity of particulars, and is capable of discovering general truths. Sense sees only the outside of things, reason acquaints itself with their natures. Sensation is only a mode of feeling in the mind; but knowledge implies an active and vital energy in the mind†."

3. The necessity of refuting Hume's inferences from the mere-sensation system led other writers to limit, in various ways, their assent to Locke. Especially was this the case with a number of intelligent metaphysicians in Scotland, as Reid, Beattie, Dugald Stewart, and Thomas Brown. Thus Reid asserts‡, "that the account which Mr. Locke himself gives of the Idea of Power cannot be reconciled to his favourite doctrine, that all our simple ideas have their origin from sensation or reflection." Reid remarks, that our memory and our reasoning power

\* P. 16.

† P. 18.

‡ *Essays on the Powers of the Human Mind*, III. 31.



come in for a share in the origin of this idea: and in speaking of reasoning, he obviously assumes the axiom that every event must have a cause. By succeeding writers of this school, the assumption of the fundamental principles, to which our nature in such cases irresistibly directs us, is more clearly pointed out. Thus Stewart defends the form of expression used by Price\*. "A variety of intuitive judgments might be mentioned, involving simple ideas, which it is impossible to trace to any origin but to the power which enables us to form these judgments. Thus it is surely an intuitive truth that the sensations of which I am conscious, and all those I remember, belong to one and the same being, which I call *myself*. Here is an intuitive judgment involving the simple idea of *Identity*. In like manner, the changes which I perceive in the universe impress me with a conviction that some cause must have operated to produce them. Here is an intuitive judgment involving the simple Idea of *Causation*. When we consider the adjacent angles made by a straight line standing upon another, and perceive that their sum is equal to two right angles, the judgment we form involves a simple idea of *Equality*. To say, therefore, that the Reason or the Understanding is a source of new ideas, is not so exceptionable a mode of speaking as has been sometimes supposed. According to Locke, *Sense* furnishes our ideas, and Reason perceives their agreements and disagreements. But the truth is, that these agreements and disagreements are in many instances, simple ideas, of which no analysis can be given; and of which the origin must therefore be referred to Reason, according to Locke's own doctrine." This view, according to which the Reason or Understanding is the source of certain simple ideas, such as Identity, Causation, Equality, which ideas are necessarily involved in the

\* *Outlines of Moral Phil.*, p. 138.

intuitive judgments which we form, when we recognize fundamental truths of science, approaches very near in effect to the doctrine which in this work we have presented, of Fundamental Ideas belonging to each science, and manifesting themselves in the axioms of the science. It may be observed, however, that by attempting to enumerate these ideas and axioms, so as to lay the foundations of the whole body of physical science; and by endeavouring, as far as possible, to simplify and connect each group of such Ideas; we have at least given a more systematic form to this doctrine. We have, moreover, traced it into many consequences to which it necessarily leads, but which do not appear to have been contemplated by the metaphysicians of the Scotch school. But I gladly acknowledge my obligations to the writers of that school; and I trust that in the near agreement of my views on such points with theirs, there is ground for believing the system of philosophy which I have in this work presented, to be that to which the minds of thoughtful men, who have meditated on such subjects, are generally tending.

4. As a further instance that such a tendency is at work, I may make a quotation from an eminent English philosophical writer of another school. "If you will be at the pains," says Archbishop Whately\*, "carefully to analyze the simplest description you hear of any transaction or state of things, you will find that the process which almost invariably takes place is, in logical language, this: that each individual *has in his mind* certain major premises or principles relative to the subject in question;—that observation of what actually presents itself to the senses, supplies minor premises; and that the statement given (and which is reported as a thing experienced) consists, in fact, of the *conclusions* drawn from the

\* *Polit. Econ.*, p. 76.

combinations of these premises." The major premises here spoken of are the Fundamental Ideas, and the Axioms and Propositions to which they lead; and whatever is regarded as a fact of observation is necessarily a conclusion in which these propositions are assumed; for these contain, as we have said, the conditions of our experience. Our experience conforms to these axioms and their consequences, whether or not the connexion be stated in a logical manner, by means of premises and a conclusion.

5. The same persuasion is also suggested by the course which the study of metaphysics has taken of late years in France. In that country, as we have seen, the Sensational System, which was considered as the necessary consequence of the revolution begun by Locke, obtained a more complete ascendancy than it did in England; and in that country too, the reaction, among metaphysical and moral writers, when its time came, was more decided and rapid than it was among Locke's own countrymen. It would appear that M. Laromiguière was one of the first to give expression to this feeling, of the necessity of a modification of the sensational philosophy. He began by professing himself the disciple of Condillac, even while he was almost unconsciously subverting the fundamental principles of that writer. And thus, as M. Cousin justly observes\*, his opinions had the more powerful effect from being presented, not as thwarting and contradicting, but as sharing and following out the spirit of his age. M. Laromiguière's work, entitled *Essai sur les Facultés de l'Ame*, consists of lectures given to the Faculty of Letters of the Academy of Paris, in the years 1811, 1812 and 1813. In the views which these lectures present, there is much which the author has in common with Condillac. But he is led by his investigation to assert†, that it is

\* *Fragmens Philosophiques*, 1. 53.

† *Ib.*, 1. 67.

not true that sensation is the sole fundamental element of our thoughts and our understanding. *Attention* also is requisite: and here we have an element of quite another kind. For sensation is passive; attention is active. Attention does not spring out of sensation; the passive principle is not the reason of the active principle. Activity and passivity are two facts entirely different. Nor can this activity be defined or derived; being, as the author says, a fundamental idea. The distinction is manifest by its own nature; and we may find evidence of it in the very forms of language. To *look* is more than to *see*; to *hearken* is more than to *hear*. The French language marks this distinction with respect to other senses also. "*On voit, et l'on regarde; on entend, et l'on écoute; on sent, et l'on flaire; on goûte, et l'on savoure.*" And thus the mere sensation, or capacity of feeling, is only the occasion on which the attention is exercised; while the attention is the foundation of all the operations of the understanding.

The reader of the former part of this work, will have seen how much we have insisted upon the activity of the mind, as the necessary basis of all knowledge. In all observation and experience, the mind is active, and by its activity apprehends all sensations in subordination to its own ideas; and thus it becomes capable of collecting knowledge from phenomena, since ideas involve general relations and connexions, which sensations of themselves cannot involve. And thus we see that, in this respect also, our philosophy stands at that point to which the speculations of the most reflective men have of late constantly been verging.

6. M. Cousin himself, from whom we have quoted the above account of Laromiguière, shares in this tendency, and has argued very energetically and successfully against the doctrines of the Sensational School. He has

made it his office once more to bring into notice among his countrymen, the doctrine of ideas as the sources of knowledge; and has revived the study of Plato, who may still be considered as one of the great leaders of the ideal school. But the larger portion of M. Cousin's works refers to questions out of the reach of our present review, and it would be unsuitable to dwell longer upon them in this place.

7. We turn to speculations more closely connected with our present subject. M. Ampère, a French man of science, well entitled by his extensive knowledge, and large and profound views, to deal with the philosophy of the sciences, published in 1834, his *Essai sur la Philosophie des Sciences, ou Exposition analytique d'une Classification Naturelle de toutes les Connaissances Humaines*. In this remarkable work we see strong evidence of the progress of the reaction against the system which derives our knowledge from sensation only. The author starts from a maxim, that in classing the sciences, we must not only regard the nature of the objects about which each science is concerned, but also the point of view under which it considers them: that is, the *ideas* which each science involves. M. Ampère also gives briefly his views of the intellectual constitution of man; a subject on which he had long and sedulously employed his thoughts; and these views are far from belonging to the Sensational School. Human thought, he says, is composed of phenomena and of conceptions. Phenomena are external, or *sensitive*; and internal, or *active*. Conceptions are of four kinds; *primitive*, as space and motion, duration and cause; *objective*, as our idea of matter and substance; *onomatic*, or those which we associate with the general terms which language presents to us; and *explicative*, by which we ascend to causes after a comparative study of phenomena. He teaches further, that in deriving ideas from sensation,

the mind is not passive; but exerts an action which, when voluntary, is called *attention*, but when it is, as it often is, involuntary, may be termed *reaction*.

I shall not dwell upon the examination of these opinions\*; but I may remark, that both in the recognition of conceptions as an original and essential element of the mind, and in giving a prominent place to the active function of the mind, in the origin of our knowledge, this view approaches to that which I have presented in the preceding part of this work; although undoubtedly with considerable differences.

8. The classification of the sciences which M. Ampère proposes, is founded upon a consideration of the sciences themselves; and is, the author conceives, in accordance with the conditions of natural classifications, as exhibited in Botany and other sciences. It is of a more symmetrical kind, and exhibits more steps of subordination, than that to which I have been led; it includes also practical Art as well as theoretical Science; and it is extended to moral and political as well as physical Sciences. It will not be necessary for me here to examine it in detail: but I may remark, that it is throughout a *dichotomous* division, each higher number being subdivided into two lower ones, and so on. In this way, M. Ampère obtains sciences of the First Order, each of which is divided into two sciences of the Second, and four of the Third Order. Thus Mechanics is divided into *Cinematics*, *Statics*, *Dynamics*, and *Molecular Mechanics*; Physics is divided into *Experimental Physics*, *Chemistry*, *Stereometry*, and *Atomology*; Geology is divided into *Physical Geography*, *Mineralogy*, *Geonomy*, and *Theory of the Earth*. Without here criticizing these divisions or their principle, I may observe that *Cinematics*, the doctrine of motion

\* See also the vigorous critique of Locke's *Essay*, by Lemaistre, *Soirées de St. Petersbourg*.

without reference to the force which produces it, is a portion of knowledge which our investigation has led us also to see the necessity of erecting into a separate science; and which we have termed *Pure Mechanism*. Of the divisions of Geology, *Physical Geography*, especially as explained by M. Ampère, is certainly a part of the subject, both important and tolerably distinct from the rest. *Geonomy* contains what we have termed in the History, *Descriptive Geology*;—the exhibition of the facts separate from the inquiry into their causes; while our *Physical Geology* agrees with M. Ampère's *Theory of the Earth*. *Mineralogy* appears to be placed by him in a different place from that which it occupies in our scheme: but in fact, he uses the term for a different science;—he applies it to the classification not of *simple minerals*, but of *rocks*, which is a science auxiliary to geology, and which has sometimes been called *Petralogy*. What we have termed *Mineralogy*, M. Ampère unites with *Chemistry*. “It belongs,” he says\*, “to Chemistry, and not to Mineralogy, to inquire how many atoms of silicium and of oxygen compose silica; to tell us that its primitive form is a rhombohedron of certain angles, that it is called *quartz*, &c.: leaving, on one hand, to Molecular Geometry the task of explaining the different secondary forms which may result from the primitive form; and on the other hand, leaving to Mineralogy the office of describing the different varieties of quartz, and the rocks in which they occur, according as the quartz is crystallized, transparent, coloured, amorphous, solid, or in sand.” But we may remark, that by adopting this arrangement, we separate from Mineralogy almost all the knowledge, and absolutely all the general knowledge, which books professing to treat of that science have usually contained. The consideration of Mineralogical

\* P. 210.

Classifications, which, as may be seen in the *History of Science*, is so curious and instructive, is forced into the domain of Chemistry, although many of the persons who figure in it were not at all properly chemists. And we lose, in this way, the advantage of that peculiar office which, in our arrangement, Mineralogy fills; of forming a rigorous transition from the sciences of classification to those which consider the mathematical properties of bodies; and connecting the external characters and the internal constitution of bodies by means of a system of important general truths. I conceive, therefore, that our disposition of this science, and our mode of applying the name, are far more convenient than those of M. Ampère.

9. We have seen the reaction against the pure sensational doctrines operating very powerfully in England and in France. But it was in Germany that these doctrines were most decidedly rejected; and systems in extreme opposition to these put forth with confidence, and received with applause. Of the authors who gave this impulse to opinions in that country, Kant was the first, and by far the most important. I have already endeavoured to explain how he was roused, by the skepticism of Hume, to examine wherein the fallacy lay which appeared to invalidate all reasonings from effect to cause; and how this inquiry terminated in a conviction that the foundations of our reasonings on this and similar points were to be sought in the mind, and not in the phenomena;—in the *subject* and not in the *object*. The revolution in the customary mode of contemplating human knowledge which Kant's opinions involved, was most complete. He himself, with no small justice, compares\* it with the change produced by Copernicus's theory of the solar system. "Hitherto," he says, "men

\* *Kritik der Reinen Vernunft*, Pref., p. xv.



have assumed that all our knowledge must be regulated by the objects of it; yet all attempts to make out anything concerning objects *à priori* by means of our conceptions," (as for instance their geometrical properties) "must, on this foundation, be unavailing. Let us then try whether we cannot make out something more in the problems of metaphysics, by assuming that objects must be regulated by our knowledge, since this agrees better with that supposition, which we are prompted to make, that we can know something of them *à priori*. This thought is like that of Copernicus, who, when he found that nothing was to be made of the phenomena of the heavens so long as everything was supposed to turn about the spectator, tried whether the matter might not be better explained if he made the spectator turn, and left the stars at rest. We may make the same essay in metaphysics, as to what concerns our intuitive knowledge respecting objects. If our apprehension of objects must be regulated by the properties of the objects, I cannot comprehend how we can possibly know anything about them *à priori*. But if the object, as apprehended by us, be regulated by the constitution of our faculties of apprehension, I can readily conceive this possibility." From this he infers that our experience must be regulated by our conceptions.

10. This view of the nature of knowledge soon superseded entirely the doctrines of the Sensational School among the metaphysicians of Germany. These philosophers did not gradually modify and reject the dogmas of Locke and Condillac, as was done in England and France\*; nor did they endeavour to ascertain the extent

\* The sensational system never acquired in Germany the ascendancy which it obtained in England and France; but I am compelled here to pass over the history of philosophy in Germany, except so far as it affects ourselves.

of the empire of Ideas by a careful survey of its several provinces, as we have been doing in the previous part of the present work. The German metaphysicians saw at once that Ideas and Things, the Subjective and the Objective elements of our knowledge, were, by Kant's system, brought into opposition and correlation, as equally real and equally indispensable. Seeing this, they rushed at once to the highest and most difficult problem of philosophy,—to determine what this correlation is;—to discover how Ideas and Things are at the same time opposite and identical;—how the world, while it is distinct from and independent of us, is yet, as an object of our knowledge, governed by the conditions of our thoughts. The attempts to solve this problem, taken in the widest sense, including the forms which it assumes in Morals, Politics, the Arts, and Religion, as well as in the Material Sciences, have, since that time, occupied the most profound speculators of Germany; and have given rise to a number of systems, which, rapidly succeeding each other, have, each in its day, been looked upon as a complete solution of the problem. To trace the characters of these various systems, does not belong to the business of the present Book: my task at present is ended when I have shown, as I have now done, how the progress of thought in the philosophical world, followed from the earliest up to the present time, has led to that recognition of the co-existence and joint necessity of the two opposite elements of our knowledge; and when I have pointed out processes adapted to the extension of our knowledge, which a true view of its nature has suggested or may suggest.

In the latter portion of my task something still remains to be done, which will be the subject of the ensuing Book.

## CHAPTER XVI.

## FURTHER ADVANCE OF THE SENSATIONAL SCHOOL\*.

I SHALL now take the liberty of noticing the views published by a contemporary writer; not that it forms part of my design to offer any criticism upon the writings of all those who have treated of those subjects on which we are now employed; but because we can more distinctly in this manner point out the contrasts and ultimate tendencies of the several systems of opinion which have come under our survey. And since from among these systems we have endeavoured to extract and secure the portion of truth which remains in each, and to reject the rest, we are led to point out the errors on which our attention is thus fixed, in recent as well as older writers.

M. Auguste Comte published in 1830 the first, and in 1835 the second volume† of his *Cours de Philosophie Positive*; of which the aim is not much different from that of the present work, since as he states, (p. viii.) such a title as the *Philosophy of the Sciences* would describe a part of his object, and would be inappropriate only by excluding that portion (not yet published) which refers to speculations concerning social relations. By employing the term *Philosophie Positive*, he wishes to distinguish the philosophy involved in the present state of our sciences from the previous forms of human knowledge. For according to him, each branch of knowledge passes, in the course of man's history, through three

\* This chapter, now first published, is printed as it was written previously to the publication of the former edition.

† I believe I had not then seen the third volume (published in 1838) or the subsequent ones.

different states; it is first *theological*, then *metaphysical*, then *positive*. By the latter term he implies a state which includes nothing but general representations of facts;—phenomena *arranged according* to relations of succession and resemblance. This “positive philosophy” rejects all inquiry after causes, which he holds to be void of sense\* and inaccessible. All such conceptions belong to the “metaphysical” state of science which deals with abstract forces, real entities, and the like. Still more completely does he reject, as altogether antiquated and absurd, the “theological” view of phenomena. Indeed he conceives† that any one’s own consciousness of what passes within himself is sufficient to convince him of the truth of the law of the three phases through which knowledge must pass. “Does not each of us,” he says, “in contemplating his own history, recollect that he has been successively a *theologian* in his infancy, a *metaphysician* in his youth, and a *physicist* in his ripe age? This may easily be verified for all men who are up to the level of their time.”

It is plain from such statements, and from the whole course of his work, that M. Comte holds, in their most rigorous form, the doctrines to which the speculations of Locke and his successors led; and which tended, as we have seen, to the exclusion of all ideas except those of number and resemblance. As M. Comte refuses to admit into his philosophy the fundamental idea of Cause, he of course excludes most of the other ideas, which are, as we endeavoured to show, the foundations of science; such as the ideas of Media by which secondary qualities are made known to us; the ideas of Chemical Attraction, of Polar Forces, and the like. He would reduce all science to the mere expression of laws of phenomena, expressed in formulæ of space, time, and number; and would condemn

\* I. p. 14.

† I. p. 7.

as unmeaning, and as belonging to an obsolete state of science, all endeavours to determine the causes of phenomena, or even to refer them to any of the other ideas just mentioned.

In a previous part of this work (B. XIII. c. vi.) I have shown, I trust decisively, that it is the genuine office of science to inquire into the causes as well as the laws of phenomena;—that such an inquiry cannot be avoided; and that it has been the source of almost all the science we possess. I need not here repeat the arguments there urged; but I may make a remark or two upon M. Comte's hypothesis, that all science is first "metaphysical" and then "positive;" since it is in virtue of this hypothesis that he rejects the investigation of causes, as worthy only of the infancy of science. All discussions concerning ideas, M. Comte would condemn as "metaphysical," and would consider as mere preludes to positive philosophy. Now I venture to assert, on the contrary, that discussions concerning ideas, and real discoveries, have in every science gone hand in hand. There is no science in which the pretended order of things can be pointed out. There is no science in which the discoveries of the laws of phenomena, when once begun, have been carried on independently of discussions concerning ideas. There is no science in which the expression of the laws of phenomena can at this time dispense with ideas which have acquired their place in science in virtue of metaphysical considerations. There is no science in which the most active disquisitions concerning ideas did not come *after*, not *before*, the first discovery of laws of phenomena. In Astronomy, the discovery of the phenomenal laws of the epicyclical motions of the heavens led to assumptions of the metaphysical principle of equable circular motions: Kepler's discoveries would never have been made but for his metaphysical

notions. These discoveries of the laws of phenomena did not lead immediately to Newton's theory, *because* a century of metaphysical discussions was requisite as a preparation. Newton then discovered, not merely a law of phenomena, but a *cause*; and *therefore* he was the greatest of discoverers. The same is the case in Optics; the ancients possessed some share of our knowledge of facts; but meddled little with the metaphysical reasonings of the subject. In modern times when men began to inquire into the *nature* of light, they soon extended their knowledge of its *laws*. When this series of discoveries had come to a pause, a new series of brilliant discoveries of laws of phenomena went on, inseparably connected with a new series of views of the nature and cause of light. In like manner, the most modern discoveries in chemistry involve indispensably the idea of polar forces. The metaphysics (in M. Comte's sense) of each subject advances in a parallel line with the knowledge of physical laws. The Explication of Conceptions must go on, as we have already shown, at the same rate as the Colligation of Facts.

M. Comte will say\* that Newton's discovery of gravitation only consists in exhibiting the astronomical phenomena of the universe as one single fact under different points of view. But this *fact* involves the idea of *force*, that is, of *cause*. And that this idea is not a mere modification of the ideas of time and space, we have shown: if it were so, how could it lead to the axiom that attraction is mutual, an indispensable part of the Newtonian theory? M. Comte says† that we do not know what attraction is, since we can only define it by identical phrases: but this is just as true of space, or time, or motion; and is in fact exactly the characteristick of a fundamental idea. We do not obtain such ideas from

\* P. 15.

† P. 16.

definitions, but we possess them not the less truly because we cannot define them.

That M. Comte's hypothesis is historically false, is obvious by such examples as I have mentioned. Metaphysical discussions have been essential steps in the progress of each science. If we arbitrarily reject all these portions of scientific history as useless trifling, belonging to the first rude attempts at knowledge, we shall not only distort the progress of things, but pervert the plainest facts. Of this we have an example in M. Comte's account of Kepler's mechanical speculations. We have seen, in the History of Physical Astronomy, that Kepler's second law, (that the planets describe areas about the sun proportional to the times,) was proved by him, by means of calculations founded on the observations of Tycho; but that the mechanical reason of it was not assigned till a later period, when it appeared as the first proposition of Newton's *Principia*. It is plain from the writings of Kepler, that it was impossible for him to show how this law resulted from the forces which were in action; since the forces which he considered were not those tending to the center, which really determine the property in question, but forces exerted by the sun *in the direction of the planet's motion*, without which forces Kepler conceived that the motion could not go on. In short, the state of mechanical science in Kepler's time was such that no demonstration of the law could be given. The terms in which such a demonstration must be expressed had not at that time acquired a precise significance; and it was in virtue of many subsequent *metaphysical* discussions (as M. Comte would term them) that these terms became capable of expressing sound mechanical reasoning. Kepler did indeed pretend to assign what he called a "physical proof" of his law, depending upon this, that the sun's force is less at greater

distances; a condition which does not at all influence the result. Thus Kepler's reason for his law proves nothing but the confusion of thought in which he was involved on such subjects. Yet M. Comte assigns to Kepler the credit of having proved this law by sound mechanical reasoning, as well as established it as a matter of fact\*. "This discovery by Kepler," he adds, "is the more remarkable, inasmuch as it occurred before the science of dynamics had really been created by Galileo." We may remark that inasmuch as M. Comte perceived this incongruity in the facts as he stated them, it is the more remarkable that he did not examine them more carefully.

The condemnation of the inquiry into causes which is conveyed in M. Comte's notion of the three stages of Science, he again expresses more in detail, in stating†

\* M. Comte's statement is so entirely at variance with the fact that I must quote it here. (*Phil. Pos.* Vol. I. p. 705.)

"Le second théorème général de dynamique consiste dans le célèbre et important *principe des aires*, dont le première idée est due à Kepler, qui découvrit et démontra forte simplement cette propriété pour le cas du mouvement d'une molécule unique, ou en d'autres terms, d'un corps dont tous les points se meuvent identiquement. Kepler établit, par les considérations les plus élémentaires, que si la force accélératrice totale dont une molécule est animée tend constamment vers un point fixé, le rayon vecteur du mobile décrit autour de ce point des aires égales en tems égaux, de telle sorte que l'aire décrite au bout d'un tems quelconque croît proportionnellement à ce tems. Il fit voir en outre que réciproquement, si une semblable relation a été vérifiée dans le mouvement d'un corps par rapport à un certain point, c'est une preuve suffisante de l'action sur le corps d'une force dirigée sans cesse vers ce point."

There is not a trace of the above propositions in the work *De Stellâ Martis*, which contains Kepler's discovery of his law, nor, I am convinced, in any other of Kepler's works. He is everywhere constant to his conceptions of the *magnetic* virtue residing in the sun, by means of which the sun, revolving on his axis, carries the planets round with him. M. Comte's statement so exactly expresses *Newton's* propositions, that one is led to suspect some extraordinary mistake, by which what should have been said of the one was transferred to the other.

† Vol. II. p. 433.



what he calls his *Fundamental theory of hypotheses*. This 'theory' is, that we may employ hypotheses in our natural philosophy, but these hypotheses must always be such as admit of a positive verification. We must have no suppositions concerning the agents by which effects are produced. All such have an anti-scientific character, and can only impede the real progress of physics. There can be no use in the ethers and imaginary fluids to which some persons refer the phenomena of heat, light, electricity and magnetism. And in agreement with this doctrine, M. Comte in his account\* of the Science of Optics, condemns, as utterly unphilosophical and absurd, both the theory of emission and that of undulation.

To this we reply, that theory of one or other kind is indispensable to the expression of the phenomena; and that when the laws are expressed, and apparently explained, by means of a theory, to forbid us to inquire whether it be really true or false, is a pedantic and capricious limitation of our knowledge, to which the intellect of man neither can nor should submit. If any one holds the adoption of one or other of these theories to be indifferent, let him express the *laws of phenomena* of diffraction in terms of the theory of emission†. If any one rejects the doctrine of undulation, let him point out some other way of connecting double refraction with polarization. And surely no man of science will contend that the beautiful branch of science which refers to that connexion is not a portion of our positive knowledge.

M. Comte's contempt for the speculations of the

\* Vol. II. p. 640.

† I venture to offer this problem;—to express the *laws of the phenomena* of diffraction without the hypothesis of undulations;—as a challenge to any one who holds such hypothesis to be unphilosophical.

undulationists seems to have prevented his acquainting himself with their reasonings, and even with the laws of phenomena on which they have reasoned, although these form by far the most striking and beautiful addition which Science has received in modern times. He adduces, as an insuperable objection to the undulatory theory, a difficulty which is fully removed by calculation in every work on the subject:—the existence of shadow\*. He barely mentions the subject of diffraction, and Young's law of interferences;—speaks of Fresnel as having applied this principle to the phenomena of coloured rings, "on which the ingenious labours of Newton left much to desire;" as if Fresnel's labours on this subject had been the supplement of those of Newton: and after regretting that "this principle of interferences has not yet been distinctly disentangled from chemical conceptions on the nature of light," concludes his chapter. He does not even mention the phenomena of dipolarization, of circular and elliptical polarization, or of the optical properties of crystals; discoveries of laws of phenomena quite as remarkable as any which can be mentioned.

M. Comte's favourite example of physical research is Thermotics, and especially Fourier's researches with regard to heat. It is shown† in the History of Thermotics, that the general phenomena of radiation required the assumption of a fluid to express them; as appears in the *theory of exchanges*‡. And the explanation of the principal laws of radiation, which Fourier gives, depends upon the conception of material molecular radiation. The *flux* of caloric, of which Fourier speaks, cannot be conceived otherwise than as implying a material flow. M. Comte apologizes§ for this expression, as too figu-

\* II. p. 641.

† II. p. 673.

‡ *Hist. Ind. Sci.* II. 489, B. x. c. i.

§ II. p. 561.

rative, and says that it merely indicates a *fact*. But what is the flow of a current of fluid except a fact? And is it not evident that without such expressions, and the ideas corresponding to them, Fourier could neither have conveyed nor conceived his theory?

In concluding this discussion, it must be recollected, that though it is a most narrow and untenable rule to say that we will admit no agency of ethers and fluids into philosophy; yet the reality of such agents is only to be held in the way, and to the extent, which the laws of phenomena indicate. It is not only allowable, but inevitable to assume, as the vehicle of heat and light, a medium possessing some of the properties of more familiar kinds of matter. But the idea of such a medium, which we possess, and on which we cannot but reason, can be fully developed only by an assiduous study of the cases in which it is applicable. It may be, that as science advances, all our knowledge may converge to one general and single aspect of the universe. We abandon and reject this hope, if we refuse to admit those ideas which must be our stepping-stones in advancing to such a point: and we no less frustrate such an expectation, if we allow ourselves to imagine that from our present position we can stride at once to the summit.

But if it is, in the sciences just mentioned, impracticable to reduce our knowledge to laws of phenomena alone, without referring to causes, media, and other agencies; how much more plainly is it impossible to confine our thoughts to phenomena, and to laws of succession and resemblance, in other sciences, as chemistry, physiology, and geology? Who shall forbid us, or why should we be forbidden, to inquire whether chemical and galvanic forces are identical; whether irritability is a peculiar vital power; whether geological causes have

been uniform or paroxysmal? To exclude such inquiries, would be to secure ourselves from the poison of error by abstaining from the banquet of truth:—it would be to attempt to feed our minds with the meagre diet of space and number, because we may find too delightful a relish in such matters as cause and end, symmetry and affinity, organization and developement.

Thus M. Comte's arrangement of the progress of science as successively metaphysical and positive, is contrary to history in fact, and contrary to sound philosophy in principle. Nor is there any better foundation for his statement that theological views are to be found only in the rude infantine condition of human knowledge, and vanish as science advances. Even in material sciences this is not the case. We have shown in the chapter on Final Causes, that physiologists have been directed in their remarks by the conviction of a purpose in every part of the structure of animals; and that this idea, which had its rise *after* the first observations, has gone on constantly gaining strength and clearness, so that it is now the basis of a large portion of the science. We have seen, too, in the Book on the palætiological sciences, that the researches of that class do by no means lead us to reject an origin of the series of events, nor to suppose this origin to be included in the series of natural laws. Science has not at all shown any reason for denying either the creation or the purpose of the universe.

This is true of those aspects of the universe which have become the subjects of rigorous science: but how small a portion of the whole do they form! Especially how minute a proportion does our knowledge bear to our ignorance, if we admit into science, as M. Comte advises, only the laws of phenomena! Even in the best explored fields of science, how few such laws do we know! Meteorology, climate, terrestrial magnetism, the

colours and other properties of bodies, the conditions of musical and articulate sound, and a thousand other facts of physics, are not defined by any known laws. In physiology we may readily convince ourselves how little we know of laws, since we can hardly study one species without discovering some unguessed property, or apply the microscope without seeing some new structure in the best known organs. And when we go on to social and moral and political matters, we may well doubt whether any one single rigorous rule of phenomena has ever been stated, although on such subjects man's ideas have been busily and eagerly working ever since his origin. What a wanton and baseless assumption it would be, then, to reject those suggestions of a Governor of the universe which we derive from man's moral and spiritual nature, and from the institutions of society, because we fancy we see in the small field of our existing 'positive knowledge' a tendency to exclude 'theological views!' Because we can explain the motion of the stars by a general Law which seems to imply no hyperphysical agency, and can trace a few more limited laws in other properties of matter, we are exhorted to reject convictions irresistibly suggested to us by our bodies and our souls, by history and antiquities, by conscience and human law.

It is not merely as a speculative doctrine that M. Comte urges the necessity of our thus following the guidance of "positive philosophy." The fevered and revolutionary condition of human society at present arises, according to him\*, from the simultaneous employment of three kinds of philosophy radically incompatible;—theological, metaphysical, and positive philosophy. The remedy for the evil is to reject the two former, and to refer everything to that positive philo-

\* I. 50.

sophy, of which the destined triumph cannot be doubtful. In like manner, our European education\*, still essentially theological, metaphysical, and literary, must be replaced by a *positive* education, suited to the spirit of our epoch.

With these practical consequences of M. Comte's philosophy we are not here concerned: but the notice of them may serve to show how entirely the rejection of the theological view pervades his system; and how closely this rejection is connected with the principles which lead him also to reject the fundamental ideas of the sciences as we have presented them.

In the detail of M. Comte's work, I do not find any peculiar or novel remarks on the induction by which the sciences are formed; except we may notice, as such, his permission of hypotheses to the enquirer, already referred to. "There can only be," he says†, "two general modes fitted to reveal to us, in a direct and entirely rational manner, the true law of any phenomenon;—either the immediate analysis of this phenomenon, or its exact and evident relation to some more extended law, previously established;—in a word, *induction*, or *deduction*. But both these ways would certainly be insufficient, even with regard to the simplest phenomenon, in the eyes of any one who fully comprehends the essential difficulties of the intimate study of nature, if we did not often begin by anticipating the result, and making a provisory supposition, at first essentially conjectural, even with respect to some of the notions which constitute the final object of enquiry. Hence the introduction, which is strictly indispensable, of hypotheses in natural philosophy." We have already seen that the "*permissio intellectus*" had been noticed as a requisite step in discovery, as long before as the time of Bacon.

\* I. 41.

† II. 433.

I do not think it necessary to examine in detail M. Comte's views of the philosophy of the different sciences; but it may illustrate the object of the present work, to make a remark upon his attempt to establish a distinction between physical and chemical science. This distinction he makes to consist in three points\* ;—that Physics considers general and Chemistry special properties;—that Physics considers masses and Chemistry molecules;—that in Physics the mode of arrangement of the molecules remains constant, while in Chemistry this arrangement is necessarily altered. M. Comte however allows that these lines of distinction are vague and insecure; for, among many others, magnetism, a special property, belongs to physics, and breaks down his first criterion; and molecular attractions are a constant subject of speculation in physics, so that the second distinction cannot be insisted on. To which we may add that the greater portion of chemistry does not attend at all to the arrangement of the molecules, so that the third character is quite erroneous. The real distinction of these branches of science is, as we have seen, the fundamental ideas which they employ. Physics deals with relations of space, time, and number, media, and scales of qualities, according to intensity and other differences; while Chemistry has for its subject elements and attractions as shown in composition; and polarity, though in different senses, belongs to both. The failure of this attempt at distinguishing these provinces of science by their objects, may be looked upon as an illustration of the impossibility of establishing a philosophy of the sciences on any other ground than the ideas which they involve.

We have thus traced to its extreme point, so far as the nature of science is concerned, one of those two

\* *Phil. Pos.* II. 392—398.

antagonist opinions, of which the struggle began in the outset of philosophy, and has continued during the whole of her progress;—namely, the opinions which respectively make our sensations and our ideas the origin of our knowledge. The former, if it be consistent with itself, must consider all knowledge of causes as impossible, since no sensation can give us the idea of cause. And when this opinion is applied to science, it reduces it to the mere investigation of laws of phenomena, according to relations. I purposely abstain, as far as possible, from the consideration of the other consequences, not strictly belonging to the physical sciences, which were drawn from the doctrine that all our ideas are only transformed sensations. The materialism, the atheism, the sensualist morality, the anarchical polity, which some of the disciples of the Sensational School erected upon the fundamental dogmas of their sect, do not belong to our present subject, and are matters too weighty to be treated of as mere accessories.

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## BOOK XIII.

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### OF METHODS EMPLOYED IN THE FORMATION OF SCIENCE.

#### CHAPTER I.

#### INTRODUCTION.

1. IN the last Book but one of this work, we pointed out certain general Characters of scientific knowledge which may often serve to distinguish it from opinions of a looser or vaguer kind. In the last Book we traced the steps by which men were led to a perception, more or less clear, of those characteristics; and in the course of this review, we had to consider various precepts and maxims offered by philosophers as fitted to guide us in the pursuit of exact and general truths. Other contributions of the same kind to the philosophy of science might be noticed, and some which contain more valuable suggestions, and indicate a more practical acquaintance with the subject than any which have yet been quoted. Among these, I must especially distinguish Sir John Herschel's *Discourse on the Study of Natural Philosophy*. But my object in this work is not so much to relate the history, as to present the really valuable results of preceding labours. I shall, therefore, proceed no further with the criticism of other authors; but shall endeavour to collect, both from them and from my own researches and reflections, such views and such rules as

seem best adapted to assist us in the discovery and recognition of scientific truth ; or, at least, such as may enable us to understand the process by which this truth is obtained. We would present to the reader the Philosophy and, if possible, the Art, of Discovery.

2. But, in truth, we must acknowledge, before we proceed with this subject, that, speaking with strictness, an *Art of Discovery* is not possible ;—that we can give no Rules for the pursuit of truth which shall be universally and peremptorily applicable ;—and that the helps which we can offer to the inquirer in such cases are limited and precarious. Still, we trust it will be found that aids may be pointed out which are neither worthless nor uninformative. The mere classification of examples of successful inquiry, to which our rules give occasion, is full of interest for the philosophical speculator. And if our maxims direct the discoverer to no operations which might not have occurred of themselves, they may still concentrate our attention on that which is most important and characteristic in these operations, and may direct us to the best mode of insuring their success. I shall, therefore, attempt to resolve the Process of Discovery into its parts, and to give an account as distinct as may be of Rules and Methods which belong to each portion of the process.

3. In the Eleventh Book we considered the three main parts of the process by which science is constructed : namely, the Decomposition and Observation of Complex Facts ; the Explication of our Ideal Conceptions ; and the Colligation of Elementary Facts by means of those Conceptions. The first and last of these three steps are capable of receiving additional accuracy by peculiar processes. They may further the advance of science in a more effectual manner, when directed by special technical *Methods*, of which in the present Book we must give a

brief view. In this more technical form, the observation of facts involves the *Measurement of Phenomena*; and the Colligation of Facts includes all arts and rules by which the process of Induction can be assisted. Hence we shall have here to consider *Methods of Observation*, and *Methods of Induction*, using these phrases in the widest sense. The second of the three steps above mentioned, the Explication of our Conceptions, does not admit of being much assisted by methods, although something may be done by Education and Discussion.

4. The Methods of Induction, of which we have to speak, apply only to the first step in our ascent from phenomena to laws of nature;—the discovery of *Laws of Phenomena*. A higher and ulterior step remains behind, and follows in natural order the discovery of Laws of Phenomena; namely, the *Discovery of Causes*; and this must be stated as a distinct and essential process in a complete view of the course of science. Again, when we have thus ascended to the causes of phenomena and of their laws, we can often reason downwards from the cause so discovered; and we are thus led to suggestions of new phenomena, or to new explanations of phenomena already known. Such proceedings may be termed *Applications* of our Discoveries; including in the phrase, *Verifications* of our Doctrines by such an application of them to observed facts. Hence we have the following series of processes concerned in the formation of science.

- (1.) Decomposition of Facts;
- (2.) Measurement of Phenomena;
- (3.) Explication of Conceptions;
- (4.) Induction of Laws of Phenomena;
- (5.) Induction of Causes;
- (6.) Application of Inductive Discoveries.

5. Of these six processes, the methods by which the

second and fourth may be assisted are here our peculiar object of attention. The treatment of these subjects in the present work must necessarily be scanty and imperfect, although we may perhaps be able to add something to what has hitherto been systematically taught on these heads. Methods of Observation and of Induction might of themselves form an abundant subject for a treatise, and hereafter probably will do so, in the hands of future writers. A few remarks, offered as contributions to this subject, may serve to show how extensive it is, and how much more ready it now is than it ever before was, for a systematic discussion.

Of the above steps of the formation of science, the first, the Decomposition of Facts, has already been sufficiently explained in the Eleventh Book: for if we pursue it into further detail and exactitude, we find that we gradually trench upon some of the succeeding parts. I, therefore, proceed to treat of the second step, the Measurement of Phenomena;—of *methods* by which this work, in its widest sense, is executed, and these I shall term Methods of Observation.

## CHAPTER II.

### OF METHODS OF OBSERVATION.

1. I SHALL speak, in this chapter, of Methods of exact and systematic observation, by which such facts are collected as form the materials of precise scientific propositions. These Methods are very various, according to the nature of the subject inquired into, and other circumstances: but a great portion of them agree in being processes of measurement. These I shall peculiarly con-

sider: and in the first place those referring to Number, Space, and Time, which are at the same time objects and instruments of measurement.

2. But though we have to explain how observations may be made as perfect as possible, we must not forget that in most cases complete perfection is unattainable. *Observations are never perfect.* For we observe phenomena by our senses, and measure their relations in time and space; but our senses and our measures are all, from various causes, inaccurate. If we have to observe the exact place of the moon among the stars, how much of instrumental apparatus is necessary! This apparatus has been improved by many successive generations of astronomers, yet it is still far from being perfect. And the senses of man, as well as his implements, are limited in their exactness. Two different observers do not obtain precisely the same measures of the time and place of a phenomenon; as, for instance, of the moment at which the moon occults a star, and the point of her *limb* at which the occultation takes place. Here, then, is a source of inaccuracy and error, even in astronomy, where the means of exact observation are incomparably more complete than they are in any other department of human research. In other cases, the task of obtaining accurate measures is far more difficult. If we have to observe the tides of the ocean when rippled with waves, we can see the average level of the water first rise and then fall; but how hard is it to select the exact moment when it is at its greatest height, or the exact highest point which it reaches! It is very easy, in such a case, to err by many minutes in time, and by several inches in space.

Still, in many cases, good Methods can remove very much of this inaccuracy, and to these we now proceed.

3. (I.) *Number*.—Number is the first step of mea-

surement, since it measures itself, and does not, like space and time, require an arbitrary standard. Hence the first exact observations, and the first advances of rigorous knowledge, appear to have been made by means of number; as for example,—the number of days in a month and in a year;—the cycles according to which eclipses occur;—the number of days in the revolutions of the planets; and the like. All these discoveries, as we have seen in the History of Astronomy, go back to the earliest period of the science, anterior to any distinct tradition; and these discoveries presuppose a series, probably a very long series, of observations, made principally by means of number. Nations so rude as to have no other means of exact measurement, have still systems of numeration by which they can reckon to a considerable extent. Very often; such nations have very complex systems, which are capable of expressing numbers of great magnitude. Number supplies the means of measuring other quantities, by the assumption of a *unit* of measure of the appropriate kind: but where nature supplies the unit, number is applicable directly and immediately. Number is an important element in the Classificatory as well as in the Mathematical Sciences. The History of those Sciences shows how the formation of botanical systems was effected by the adoption of number as a leading element by Cæsalpinus; and how afterwards the Reform of Linnæus in classification depended in a great degree on his finding, in the pistils and stamens, a better numerical basis than those before employed. In like manner, the number of rays in the membrane of the gills\*, and the number of rays in the fins of fish, were found to be important elements in ichthyological classification by Artedi and Linnæus. There are innumerable instances, in all parts of Natural

\* *Hist. Ind. Sci.*, B. xvi. c. vii.

History, of the importance of the observation of number. And in this observation, no instrument, scale or standard is needed, or can be applied; except the scale of natural numbers, expressed either in words or in figures, can be considered as an instrument.

4. (II.) *Measurement of Space*.—Of quantities admitting of *continuous* increase and decrease, (for number is discontinuous,) space is the most simple in its mode of measurement, and requires most frequently to be measured. The obvious mode of measuring space is by the repeated application of a material measure, as when we take a foot-rule and measure the length of a room. And in this case the foot-rule is the *unit* of space, and the length of the room is expressed by the number of such units which it contains: or, as it may not contain an exact number, by a number with a *fraction*. But besides this measurement of linear space, there is another kind of space which, for purposes of science, it is still more important to measure, namely, angular space. The visible heavens being considered as a sphere, the portions and paths of the heavenly bodies are determined by drawing circles on the surface of this sphere, and are expressed by means of the parts of these circles thus intercepted: by such measures the doctrines of astronomy were obtained in the very beginning of the science. The arcs of circles thus measured, are not like linear spaces, reckoned by means of an *arbitrary* unit; for there is a *natural unit*, the total circumference, to which all arcs may be referred. For the sake of convenience, the whole circumference is divided into 360 parts or *degrees*; and by means of these degrees and their parts, all arcs are expressed. The *arcs* are the measures of the *angles at the center*, and the degrees may be considered indifferently as measuring the one or the other of these quantities.

5. In the History of Astronomy\*, I have described the method of observation of celestial angles employed by the Greeks. They determined the lines in which the heavenly bodies were seen, by means either of Shadows, or of Sights; and measured the angles between such lines by arcs or rules properly applied to them. The Armill, Astrolabe, Dioptra, and Parallactic Instrument of the ancients, were some of the instruments thus constructed. Tycho Brahe greatly improved the methods of astronomical observation by giving steadiness to the frame of his instruments, (which were large *quadrants*,) and accuracy to the divisions of the *limb*†. But the application of the *telescope* to the astronomical quadrant and the fixation of the center of the field by a *cross* of fine wires placed in the focus, was an immense improvement of the instrument, since it substituted a precise visual ray, pointing to the star, instead of the coarse coincidence of Sights. The accuracy of observation was still further increased by applying to the telescope a *micrometer* which might subdivide the smaller divisions of the arc.

6. By this means, the precision of astronomical observation was made so great, that very minute angular spaces could be measured: and it then became a question whether discrepancies which appeared at first as defects in the theory, might not arise sometimes from a bending or shaking of the instrument, and from the degrees marked on the limb being really somewhat unequal, instead of being rigorously equal. Accordingly, the framing and balancing of the instrument, so as to avoid all possible tremor or flexure, and the exact division of an arc into equal parts, became great objects of those who wished to improve astronomical observations. The observer no longer gazed at the stars from a lofty

\* *Hist. Ind. Sci.*, B. III. c. iv. sect. 3.

† *Ibid.*, B. VII. c. vi. sect. 1.



tower, but placed his telescope on the solid ground, and braced and balanced it with various contrivances. Instead of a quadrant, an entire circle was introduced (by Ramsden;) and various processes were invented for the dividing of instruments. Among these we may notice Troughton's method of dividing; in which the visual ray of a microscope was substituted for the points of a pair of compasses, and, by *stepping* round the circle, the partial arcs were made to bear their exact relation to the whole circumference.

7. Astronomy is not the only science which depends on the measurement of angles. Crystallography also requires exact measures of this kind; and the *goniometer*, especially that devised by Wollaston, supplies the means of obtaining such measures. The science of Optics also, in many cases, requires the measurement of angles.

8. In the measurement of linear space, there is no natural standard which offers itself. Most of the common measures appear to be taken from some part of the human body; as a *foot*, a *cubit*, a *fathom*; but such measures cannot possess any precision, and are altered by convention: thus there were in ancient times many kinds of cubits; and in modern Europe, there are a great number of different standards of the foot, as the Rhenish foot, the Paris foot, the English foot. It is very desirable that, if possible, some permanent standard, founded in nature, should be adopted; for the conventional measures are lost in the course of ages; and thus, dimensions expressed by means of them become unintelligible. Two different natural standards have been employed in modern times: the French have referred their measures of length to the total circumference of a meridian of the earth; a quadrant of this meridian consists of ten million units or *metres*. The English

have fixed their linear measure by reference to the length of a pendulum which employs an exact second of time in its small oscillation. Both these methods occasion considerable difficulties in carrying them into effect; and are to be considered mainly as means of recovering the standard if it should ever be lost. For common purposes, some material standard is adopted as authority for the time: for example, the standard which in England possessed legal authority up to the year 1835 was preserved in the House of Parliament; and was lost in the conflagration which destroyed that edifice. The standard of length now generally referred to by men of science in England is that which is in the possession of the Astronomical Society of London.

9. A standard of length being established, the artifices for applying it, and for subdividing it in the most accurate manner, are nearly the same as in the case of measures of arcs: as for instance, the employment of the visual rays of microscopes instead of the legs of compasses and the edges of rules; the use of micrometers for minute measurements; and the like. Many different modes of avoiding error in such measurements have been devised by various observers, according to the nature of the cases with which they had to deal\*.

10. (III.) *Measurement of Time*.—The methods of measuring Time are not so obvious as the methods of measuring space; for we cannot apply one portion of time to another, so as to test their equality. We are obliged to begin by assuming some change as the measure of time. Thus the motion of the sun in the sky, or the length and position of the shadows of objects, were the first modes of measuring the parts of the day. But

\* On the precautions employed in astronomical instruments for the measure of space, see Sir J. Herschel's *Astronomy*, (in the *Cabinet Cyclopædia*,) Arts. 103—110.

what assurance had men, or what assurance could they have, that the motion of the sun or of the shadow was uniform? They could have no such assurance, till they had adopted some measure of smaller times; which smaller times, making up larger times by repetition, they took as the standard of uniformity;—for example, an hour-glass, or a clepsydra which answered the same purpose among the ancients. There is no apparent reason why the successive periods measured by the emptying of the hour-glass should be unequal; they are implicitly accepted as equal; and by reference to these, the uniformity of the sun's motion may be verified. But the great improvement in the measurement of time was the use of a pendulum for the purpose by Galileo, and the application of this device to clocks by Huyghens in 1656. For the successive oscillations of a pendulum are rigorously equal, and a clock is only a train of machinery employed for the purpose of counting these oscillations. By means of this invention, the measure of time in astronomical observations became as accurate as the measure of space.

11. What is the *natural unit* of time? It was assumed from the first by the Greek astronomers, that the sidereal days, measured by the revolution of a star from any meridian to the same meridian again, are exactly equal; and all improvements in the measure of time tended to confirm this assumption. The sidereal day is therefore the natural standard of time. But the solar day, determined by the diurnal revolution of the sun, although not rigorously invariable, as the sidereal day is, undergoes scarcely any perceptible variation; and since the course of daily occurrences is regulated by the sun, it is far more convenient to seek the basis of our unit of time in *his* motions. Accordingly the solar day (the *mean* solar day) is divided into 24 hours, and these, into minutes and seconds; and this is our scale of time.

Of such time, the sidereal day has 23 hours 56 minutes 4.09 seconds. And it is plain that by such a statement the length of the hour is fixed, with reference to a sidereal day. The *standard* of time (and the standard of space in like manner) equally answers its purpose, whether or not it coincides with any *whole number* of units.

12. Since the sidereal day is thus the standard of our measures of time, it becomes desirable to refer to it, constantly and exactly, the instruments by which time is measured, in order that we may secure ourselves against error. For this purpose, in astronomical observatories, observations are constantly made of the transit of stars across the meridian; the *transit instrument* with which this is done being adjusted with all imaginable regard to accuracy\*.

13. When exact measures of time are required in other than astronomical observations, the same instruments are still used, namely, clocks and chronometers. In chronometers, the regulating part is an oscillating body; not, as in clocks, a pendulum oscillating by the force of gravity, but a wheel swinging to and fro on its center, in consequence of the vibrations of a slender coil of elastic wire. To divide time into still smaller portions than these vibrations, other artifices are used; some of which will be mentioned under the next head.

14. (IV.) *Conversion of Space and Time.*—Space and time agree in being extended quantities, which are made up and measured by the repetition of homogeneous parts. If a body move uniformly, whether in the way of revolving or otherwise, the *space* which any point describes, is *proportional* to the *time* of its motion; and the space and the time may each be taken as a measure

\* On the precautions employed in the measure of time by astronomers, see Herschel's *Astron.*, Art. 115—127.

of the other. Hence in such cases, by taking space instead of time, or time instead of space, we may often obtain more convenient and precise measures, than we can by measuring directly the element with which we are concerned.

The most prominent example of such a conversion, is the measurement of the Right Ascension of stars, (that is, their angular distance from a standard meridian\* on the celestial sphere,) by means of the time employed in their coming to the meridian of the place of observation. Since, as we have already stated, the visible celestial sphere, carrying the fixed stars, revolves with perfect uniformity about the pole; if we observe the stars as they come in succession to a fixed circle passing through the poles, the intervals of time between these observations will be proportional to the angles which the meridian circles passing through these stars make at the poles where they meet; and hence, if we have the means of measuring time with great accuracy, we can, by watching the *times* of the transits of successive stars across some visible mark in our own meridian, determine the *angular distances* of the meridian circles of all the stars from one another.

Accordingly, now that the pendulum clock affords astronomers the means of determining time exactly, a measurement of the Right Ascensions of heavenly bodies by means of a clock and a transit instrument, is a part of the regular business of an observatory. If the sidereal clock be so adjusted that it marks the beginning of its scale of time when the first point of Right Ascension is upon the visible meridian of our observatory, the point of the scale at which the clock points when any other

\* A *meridian* is a circle passing through the poles about which the celestial sphere revolves. The meridian of *any place* on the earth is that meridian which is exactly over the place.

star is in our meridian, will truly represent the Right Ascension of the star.

Thus as the motion of the stars is our measure of time, we employ time, conversely, as our measure of the places of the stars. The celestial machine and our terrestrial machines correspond to each other in their movements; and the star steals silently and steadily across our meridian line, just as the pointer of the clock steals past the mark of the hour. We may judge of the scale of this motion by considering that the full moon employs about two minutes of time in sailing across any fixed line seen against the sky, transverse to her path: and all the celestial bodies, carried along by the revolving sphere, travel at the same rate.

15. In this case, up to a certain degree, we render our measures of astronomical angles more exact and convenient by substituting time for space; but when, in the very same kind of observation, we wish to proceed to a greater degree of accuracy, we find that it is best done by substituting space for time. In observing the transit of a star across the meridian, if we have the clock within hearing, we can count the beats of the pendulum by the noise which they make, and tell exactly at which second of time the passage of the star across the visible thread takes place; and thus we measure Right Ascension by means of time. But our perception of time does not allow us to divide a second into ten parts, and to pronounce whether the transit takes place three-tenths, six-tenths, or seven-tenths of a second after the preceding beat of the clock. This, however, can be done by the usual mode of observing the transit of a star. The observer, listening to the beat of his clock, fastens his attention upon the star at each beat, and especially at the one immediately before and the one immediately after the passage of the thread: and by this means he

has these two positions and the positions of the thread so far present to his intuition at once, that he can judge in what proportion the thread is nearer to one position than the other, and can thus divide the intervening second in its due proportion. Thus if he observe that at the beginning of the second the star is on one side of the thread, and at the end of the second on the other side; and that the two distances from the thread are as two to three, he knows that the transit took place at two-fifths (or four-tenths) of a second after the former beat. In this way a second of time in astronomical observations may, by a skilful observer, be divided into ten equal parts; although when time is observed as time, a tenth of a second appears almost to escape our senses. From the above explanation, it will be seen that the reason why the subdivision is possible in the way thus described, is this:—that the moment of time thus to be divided is so small, that the eye and the mind can retain, to the end of this moment, the impression of position which it received at the beginning. Though the two positions of the star, and the intermediate thread, are seen successively, they can be contemplated by the mind as if they were seen simultaneously: and thus it is precisely the smallness of this portion of time which enables us to subdivide it by means of space.

16. There is another case, of somewhat a different kind, in which time is employed in measuring space; namely, when space, or the standard of space, is defined by the length of a pendulum oscillating in a given time. We might in this way define any space by the time which a pendulum of such a length would take in oscillating; and thus we might speak, as was observed by those who suggested this device, of five minutes of cloth, or a rope half an hour long. We may observe, however, that in this case, the space is *not proportional* to the time. And

we may add, that though we thus appear to avoid the arbitrary standard of space (for as we have seen, the standard of measures of time is a natural one,) we do not do so in fact: for we assume the invariableness of gravity, which really varies (though very slightly,) from place to place.

17. (V.) *The Method of Repetition in Measurement.*  
—In many cases we can give great additional accuracy to our measurements by repeatedly adding to itself the quantity which we wish to measure. Thus if we wished to ascertain the exact breadth of a thread, it might not be easy to determine whether it was one-ninetieth, or one-ninety-fifth, or one-hundredth part of an inch; but if we find that ninety-six such threads placed side by side occupy exactly an inch, we have the precise measure of the breadth of the thread. In the same manner, if two clocks are going nearly at the same rate, we may not be able to distinguish the excess of an oscillation of one of the pendulums over an oscillation of the other: but when the two clocks have gone for an hour, one of them may have gained ten seconds upon the other; thus showing that the proportion of their times of oscillation is 3610 to 3600.

In the latter of these instances, we have the principle of repetition truly exemplified, because (as has been justly observed by Sir J. Herschel\*,) there is then “a juxtaposition of units without error,”—“one vibration commences exactly where the last terminates, no part of time being lost or gained in the addition of the units so counted.” In space, this juxtaposition of units without error cannot be rigorously accomplished, since the units must be added together by material contact (as in the case of the threads,) or in some equivalent manner. Yet the principle of repetition has been applied to angular

\* *Disc. Nat. Phil.*, Art. 121.



measurement with considerable success in Borda's Repeating Circle. In this instrument, the angle between two objects which we have to observe, is repeated along the graduated limb of the circle by turning the telescope from one object to the other, alternately fastened to the circle (by its *clamp*) and loose from it (by unclamping). In this manner the errors of graduation may (theoretically) be entirely got rid of: for if an angle repeated *nine* times be found to go twice round the circle, it must be *exactly* eighty degrees: and where the repetition does not give an exact number of circumferences, it may still be made to subdivide the error to any required extent.

18. Connected with the principle of repetition, is the *Method of coincidences or interferences*. If we have two Scales, on one of which an inch is divided into 10, and on the other into 11 equal parts; and if, these Scales being placed side by side, it appear that the beginning of the latter Scale is between the 2nd and 3rd division of the former, it may not be apparent what fraction added to 2 determines the place of beginning of the second Scale as measured on the first. But if it appear also that the 3rd division of the second Scale *coincides* with a certain division of the first, (the 5th,) it is certain that 2 and *three-tenths* is the *exact* place of the beginning of the second Scale, measured on the first Scale. The 3rd division of the 11 Scale will coincide (or interfere with) a division of the 10 Scale, when the beginning or *zero* of the 11 divisions is three-tenths of a division beyond the preceding line of the 10 Scale; as will be plain on a little consideration. And if we have two Scales of equal units, in which each unit is divided into nearly, but not quite, the same number of equal parts (as 10 and 11, 19 and 20, 29 and 30,) and one sliding on the other, it will always happen that some one or other of the division lines will coincide, or very nearly coincide; and thus the

exact position of the beginning of one unit, measured on the other scale, is determined. A sliding scale, thus divided for the purpose of subdividing the units of that on which it slides, is called a *Vernier*, from the name of its inventor.

19. The same Principle of Coincidence or Interference is applied to the exact measurement of the length of time occupied in the oscillation of a pendulum. If a detached pendulum, of such a length as to swing in little less than a second, be placed before the seconds' pendulum of a clock, and if the two pendulums begin to move together, the former will gain upon the latter, and in a little while their motions will be quite discordant. But if we go on watching, we shall find them, after a time, to agree again exactly; namely, when the detached pendulum has gained one complete oscillation (back and forwards,) upon the clock pendulum, and again coincides with it in its motion. If this happen after 5 minutes, we know that the times of oscillation of the two pendulums are in the proportion of 300 to 302, and therefore the detached pendulum oscillates in  $1\frac{5}{31}$  of a second. The accuracy which can be obtained in the measure of an oscillation by this means is great; for the clock can be compared (by observing transits of the stars or otherwise) with the natural standard of time, the sidereal day. And the moment of coincidence of the two pendulums may, by proper arrangements, be very exactly determined.

We have hitherto spoken of methods of measuring time and space, but other elements also may be very precisely measured by various means.

20. (VI.) *Measurement of Weight*.—Weight, like space and time, is a quantity made up by addition of parts, and may be measured by similar methods. The principle of repetition is applicable to the measurement of weight; for if two bodies be put in the same pan of a

balance and balances the same pieces in the other pan, their weights are exactly added.

There may be difficulties of practical workmanship in carrying into effect the mathematical conditions of a perfect balance; for example, in securing an exact equality of the effective arms of the beam in all positions. These difficulties are evaded by the *Method of double weighing*; according to which the standard weights, and the body which is to be weighed, are successively put in the *same* pan, and made to balance by a third body in the opposite scale. By this means the different lengths of the arms of the beam, and other imperfections of the balance, become of no consequence\*.

21. There is no natural *Standard* of weight. The conventional weight taken as the standard, is the weight of a given bulk of some known substance; for instance, a *cubic foot of water*. But in order that this may be definite, the water must not contain any portion of heterogeneous substance: hence it is required that the water be *distilled* water.

22. (VII.) *Measurement of Secondary Qualities*.—We have already seen\* that secondary qualities are estimated by means of conventional Scales, which refer them to space, number, or some other definite expression. Thus the Thermometer measures heat; the Musical Scale, with or without the aid of number, expresses the pitch of a note; and we may have an exact and complete Scale of Colours, pure and impure. We may remark, however, that with regard to sound and colour, the estimates of the ear and the eye are not superseded, but only assisted: for if we determine what a note is, by

\* For other methods of measuring weights accurately, see Faraday's *Chemical Manipulation*, p. 25.

† Book III. c. ii. Of the Measure of Secondary Qualities.

comparing it with an instrument known to be in tune, we still leave the ear to decide when the note is *in unison* with one of the notes of the instrument. And when we compare a colour with our chromatometer, we judge by the eye which division of the chromatometer it *matches*. Colour and sound have their Natural Scales, which the eye and ear habitually apply; what science requires is, that those scales should be systematized. We have seen that several conditions are requisite in such scales of qualities: the observer's skill and ingenuity are mainly shown in devising such scales and methods of applying them.

23. The Method of Coincidences is employed in harmonics: for if two notes are nearly, but not quite, in unison, the coincidences of the vibrations produce an audible undulation in the note, which is called the *howl*; and the exactness of the unison is known by this howl vanishing.

24. (VIII.) *Manipulation*.—The process of applying practically methods of experiment and observation, is termed Manipulation; and the value of observations depends much upon the proficiency of the observer in this art. This skill appears, as we have said, not only in devising means and modes of measuring results, but also in inventing and executing arrangements by which elements are subjected to such conditions as the investigation requires: in finding and using some material combination by which nature shall be asked the question which we have in our minds. To do this in any subject may be considered as a peculiar Art, but especially in Chemistry; where “many experiments, and even whole trains of research, are essentially dependent for success on mere manipulation\*.” The changes which the chemist has to study,—compositions, decompositions, and mutual actions,

\* Faraday's *Chemical Manipulation*, p. 3.

affecting the internal structure rather than the external form and motion of bodies,—are not familiarly recognized by common observers, as those actions are which operate upon the total mass of a body: and hence it is only when the chemist has become, to a certain degree, familiar with his science, that he has the power of observing. He must learn to interpret the effects of mixture, heat, and other chemical agencies, so as to see in them those facts which chemistry makes the basis of her doctrines. And in learning to interpret this language, he must also learn to call it forth;—to place bodies under the requisite conditions, by the apparatus of his own laboratory and the operations of his own fingers. To do this with readiness and precision, is, as we have said, an Art, both of the mind and of the hand, in no small degree recondite and difficult. A person may be well acquainted with all the doctrines of chemistry, and may yet fail in the simplest experiment. How many precautions and observances, what resource and invention, what delicacy and vigilance, are requisite in Chemical Manipulation, may be seen by reference to Dr. Faraday's work on that subject.

25. The same qualities in the observer are requisite in some other departments of science; for example, in the researches of Optics: for in these, after the first broad facts have been noticed, the remaining features of the phenomena are both very complex and very minute; and require both ingenuity in the invention of experiments, and a keen scrutiny of their results. We have instances of the application of these qualities in most of the optical experimenters of recent times, and certainly in no one more than Sir David Brewster. Omitting here all notice of his succeeding labours, his *Treatise on New Philosophical Instruments*, published in 1813, is an excellent model of the kind of resource and skill of

which we now speak. I may mention as an example of this skill, his mode of determining the refractive power of an *irregular* fragment of any transparent substance. At first this might appear an impossible problem; for it would seem that a regular and smooth surface are requisite, in order that we may have any measurable refraction. But Sir David Brewster overcame the difficulty by immersing the fragment in a combination of fluids, so mixed, that they had the same refractive power as the specimen. The question, *when* they had this power, was answered by noticing when the fragment became so transparent that its surface could hardly be seen; for this happened when, the refractive power within and without the fragment being the same, there was no refraction at the surface. And this condition being obtained, the refractive power of the fluid, and therefore of the fragment, was easily ascertained.

26. (IX.) *The Education of the Senses*.—Colour and Musical Tone are, as we have seen, determined by means of the Senses, whether or not Systematical Scales are used in expressing the observed fact. Systematical Scales of sensible qualities, however, not only give precision to the record, but to the observation. But for this purpose such an Education of the Senses is requisite as may enable us to apply the scale immediately. The memory must retain the sensation or perception to which the technical term or degree of the scale refers. Thus with regard to colour, as we have said already\*, when we find such terms as *tin-white* or *pinchbeck-brown*, the metallic colour so denoted ought to occur at once to our recollection without delay or search. The observer's senses, therefore, must be educated, at first by an actual exhibition of the standard, and afterwards by a familiar use of it, to understand readily and clearly each phrase

\* Book VIII. c. iii. Terminology.

and degree of the scales which in his observations he has to apply. This is not only the best, but in many cases the only way in which the observation can be expressed. Thus *glassy lustre*, *fatty lustre*, *adamantine lustre*, denote certain kinds of shining in minerals, which appearances we should endeavour in vain to describe by periphrasis; and which the terms, if considered as terms in common language, would by no means clearly discriminate: for who, in common language, would say that coal has a fatty lustre? But these terms, in their conventional sense, are perfectly definite; and when the eye is once familiarized with this application of them, are easily and clearly intelligible.

27. The education of the senses, which is thus requisite in order to understand well the terminology of any science, must be acquired by an inspection of the objects which the science deals with; and is, perhaps, best promoted by the practical study of Natural History. In the different departments of Natural History, the descriptions of species are given by means of an extensive technical *terminology*: and that education of which we now speak, ought to produce the effect of making the observer as familiar with each of these terms as we are with the words of our common language. The technical terms have a much more precise meaning than other terms, since they are defined by express convention, and not learnt by common usage merely. Yet though they are thus defined, not the definition, but the perception itself, is that which the term suggests to the proficient.

In order to use the terminology to any good purpose, the student must possess it, not as a dictionary, but as a language. The terminology of his sciences must be the natural historian's most familiar tongue. He must learn to think in such language. And when this is achieved, the terminology, as I have elsewhere said, though to an

uneducated eye cumbrous and pedantical, is felt to be a useful implement, not an oppressive burden\*. The impatient schoolboy looks upon his grammar and vocabulary as irksome and burdensome; but the accomplished student who has learnt the language by means of them, knows that they have given him the means of expressing what he thinks, and even of thinking more precisely. And as the study of language thus gives precision to the thoughts, the study of Natural History, and especially of the descriptive part of it, gives precision to the senses.

The Education of the Senses is also greatly promoted by the practical pursuit of any science of experiment and observation, as chemistry or astronomy. The methods of manipulating, of which we have just spoken, in chemistry, and the methods of measuring extremely minute portions of space and time which are employed in astronomy, and which are described in the former part of this chapter, are among the best modes of educating the senses for purposes of scientific observation.

28. By the various Methods of precise observation which we have thus very briefly described, facts are collected, of an exact and definite kind; they are then bound together in general laws, by the aid of general ideas and of such methods as we have now to consider. It is true, that the ideas which enable us to combine facts into general propositions, do commonly operate in our minds while we are still engaged in the office of observing. Ideas of one kind or other are requisite to connect our phenomena into facts, and to give meaning to the terms of our descriptions: and it frequently happens, that long before we have collected all the facts which induction requires, the mind catches the suggestion which some of these ideas offer, and leaps forwards to a conjectural law

\* *Hist. Ind. Sci.*, B. xvi. c. iv. sect. 2.



while the labour of observation is yet unfinished. But though this actually occurs, it is easy to see that the process of combining and generalizing facts is, in the order of nature, posterior to, and distinct from, the process of observing facts. Not only is this so, but there is an intermediate step which, though inseparable from all successful generalization, may be distinguished from it in our survey; and may, in some degree, be assisted by peculiar methods. To the consideration of such methods we now proceed.

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### CHAPTER III.

#### OF METHODS OF ACQUIRING CLEAR SCIENTIFIC IDEAS; *and first* OF INTELLECTUAL EDUCATION.

THE ways in which men become masters of those clear and yet comprehensive conceptions which the formation and reception of science require, are mainly two; which, although we cannot reduce them to any exact scheme, we may still, in a loose use of the term, call *Methods* of acquiring clear Ideas. These two ways are Education and Discussion.

1. (I.) *Idea of Space*.—It is easily seen that Education may do at least something to render our ideas distinct and precise. To learn Geometry in youth, tends, manifestly, to render our idea of space clear and exact. By such an education, all the relations, all the consequences of this idea, come to be readily and steadily apprehended; and thus it becomes easy for us to understand portions of science which otherwise we should by no means be able to comprehend. The conception of *similar triangles* was to be mastered, before the disciples of Thales could see

the validity of his method of determining the height of lofty objects by the length of their shadows. The conception of *the sphere with its circles* had to become familiar, before the annual motion of the sun and its influence upon the lengths of days could be rightly traced. The properties of circles, combined with the *pure\* doctrine of motion*, were required as an introduction to the theory of Epicycles: the properties of *conic sections* were needed, as a preparation for the discoveries of Kepler. And not only was it necessary that men should possess a *knowledge* of certain figures and their properties; but it was equally necessary that they should have the *habit of reasoning* with perfect steadiness, precision, and conclusiveness concerning the relations of space. No small discipline of the mind is requisite, in most cases, to accustom it to go, with complete insight and security, through the demonstrations respecting intersecting planes and lines, dihedral and trihedral angles, which occur in solid geometry. Yet how absolutely necessary is a perfect mastery of such reasonings, to him who is to explain the motions of the moon in latitude and longitude! How necessary, again, is the same faculty to the student of crystallography! Without mathematical habits of conception and of thinking, these portions of science are perfectly inaccessible. But the early study of plane and solid geometry gives to all tolerably gifted persons, the habits which are thus needed. The discipline of following the reasonings of didactic works on this subject, till we are quite familiar with them, and of devising for ourselves reasonings of the same kind, (as, for instance, the solutions of problems proposed,) soon gives the mind the power of *discoursing* with perfect facility concerning the most complex and multiplied relations of space, and enables us to refer to the properties of all plane and solid figures as surely as

\* See Book II. c. xiii.

to the visible forms of objects. Thus we have here a signal instance of the efficacy of education in giving to our Conceptions that clearness, which the formation and existence of science indispensably require.

2. It is not my intention here to enter into the details of the form which should be given to education, in order that it may answer the purposes now contemplated. But I may make a remark, which the above examples naturally suggest, that in a mathematical education, considered as a preparation for furthering or understanding physical science, Geometry is to be cultivated, far rather than Algebra:—the properties of space are to be studied and reasoned upon as they are in themselves, not as they are replaced and disguised by symbolical representations. It is true, that when the student is become quite familiar with elementary geometry, he may often enable himself to deal in a more rapid and comprehensive manner with the relations of space, by using the language of symbols and the principles of symbolical calculation: but this is an ulterior step, which may be added to, but can never be substituted for, the direct cultivation of geometry. The method of symbolical reasoning employed upon subjects of geometry and mechanics, has certainly achieved some remarkable triumphs in the treatment of the theory of the universe. These successful applications of symbols in the highest problems of physical astronomy appear to have made some teachers of mathematics imagine that it is best to *begin* the pupil's course with such symbolical generalities. But this mode of proceeding will be so far from giving the student clear ideas of mathematical relations, that it will involve him in utter confusion, and probably prevent his ever obtaining a firm footing in geometry. To commence mathematics in such a way, would be much as if we should begin the study of a language by reading the highest strains of its lyrical poetry.

3. (II.) *Idea of Number, &c.*—The study of mathe-

matics, as I need hardly observe, develops and renders exact, our conceptions of the relations of number, as well as of space. And although, as we have already noticed, even in their original form the conceptions of number are for the most part very distinct, they may be still further improved by such discipline. In complex cases, a methodical cultivation of the mind in such subjects is needed: for instance, questions concerning cycles, and intercalations, and epacts, and the like, require very great steadiness of arithmetical apprehension in order that the reasoner may deal with them rightly. In the same manner, a mastery of problems belonging to the science of Pure Motion, or, as I have termed it, Mechanism, requires either great natural aptitude in the student, or a mind properly disciplined by suitable branches of mathematical study.

4. Arithmetic and Geometry have long been standard portions of the education of cultured persons throughout the civilized world; and hence all such persons have been able to accept and comprehend those portions of science which depend upon the idea of space: for instance, the doctrine of the globular form of the earth, with its consequences, such as the measures of latitude and longitude;—the heliocentric system of the universe in modern, or the geocentric in ancient times;—the explanation of the rainbow; and the like. In nations where there is no such education, these portions of science cannot exist as a part of the general stock of the knowledge of society, however intelligently they may be pursued by single philosophers dispersed here and there in the community.

5. (III.) *Idea of Force*.—As the idea of Space is brought out in its full evidence by the study of Geometry, so the idea of Force is called up and developed by the study of the science of Mechanics. It has already been shown, in our scrutiny of the Ideas of the Mechanical

Sciences, that Force, the Cause of motion or of equilibrium, involves an independent Fundamental Idea, and is quite incapable of being resolved into any mere modification of our conceptions of space, time, and motion. And in order that the student may possess this idea in a precise and manifest shape, he must pursue the science of Mechanics in the mode which this view of its nature demands;—that is, he must study it as an independent science, resting on solid elementary principles of its own, and not built upon some other unmechanical science as its substructure. He must trace the truths of Mechanics from their own axioms and definitions; these axioms and definitions being considered as merely means of bringing into play the Idea on which the science depends. The conceptions of force and matter, of action and reaction, of momentum and inertia, with the reasonings in which they are involved, cannot be evaded by any substitution of lines or symbols for the conceptions. Any attempts at such substitution would render the study of Mechanics useless as a preparation of the mind for physical science; and would, indeed, except counteracted by great natural clearness of thought on such subjects, fill the mind with confused and vague notions, quite unavailing for any purposes of sound reasoning. But, on the other hand, the study of Mechanics, in its genuine form, as a branch of education, is fitted to give a most useful and valuable precision of thought on such subjects; and is the more to be recommended, since, in the general habits of most men's minds, the mechanical conceptions are tainted with far greater obscurity and perplexity than belongs to the conceptions of number, space, and motion.

6. As habitually distinct conceptions of space and motion were requisite for the reception of the doctrines of formal astronomy, (the Ptolemaic and Copernican

system,) so a clear and steady conception of force is indispensably necessary for understanding the Newtonian system of physical astronomy. It may be objected that the study of Mechanics as a science has not commonly formed part of a liberal education in Europe, and yet that educated persons have commonly accepted the Newtonian system. But to this we reply, that although most persons of good intellectual culture have professed to assent to the Newtonian system of the universe, yet they have, in fact, entertained it in so vague and perplexed a manner as to show very clearly that a better mental preparation than the usual one is necessary, in order that such persons may really understand the doctrine of universal attraction. I have already spoken of the prevalent indistinctness of mechanical conceptions\*; and need not here dwell upon the indications, constantly occurring in conversation and in literature, of the utter inaccuracy of thought on such subjects which may often be detected; for instance, in the mode in which many men speak of centrifugal and centripetal forces;—of projectile and central forces;—of the effect of the moon upon the waters of the ocean; and the like. The incoherence of ideas which we frequently witness on such points, shows us clearly that, in the minds of a great number of men, well educated according to the present standard, the acceptance of the doctrine of universal gravitation is a result of traditional prejudice, not of rational conviction. And those who are Newtonians on such grounds, are not at all more intellectually advanced by being Newtonians in the nineteenth century, than they would have been by being Ptolemaics in the fifteenth.

7. It is undoubtedly in the highest degree desirable that all great advances in science should become the

\* B. III. c. x.

common property of all cultivated men. And this can only be done by introducing into the course of a liberal education such studies as unfold and fix in men's minds the fundamental ideas upon which the new-discovered truths rest. The progress made by the ancients in geography, astronomy, and other sciences, led them to assign, wisely and well, a place to arithmetic and geometry among the steps of an ingenuous education. The discoveries of modern times have rendered these steps still more indispensable; for we cannot consider a man as cultivated up to the standard of his times, if he is not only ignorant of, but incapable of comprehending, the greatest achievements of the human intellect. And as innumerable discoveries of all ages have thus secured to Geometry her place as a part of good education, so the great discoveries of Newton make it proper to introduce Elementary Mechanics as a part of the same course. If the education deserve to be called good, the pupil will not remain ignorant of those discoveries, the most remarkable extensions of the field of human knowledge which have ever occurred. Yet he cannot by possibility comprehend them, except his mind be previously disciplined by mechanical studies. The period appears now to be arrived when we may venture, or rather when we are bound to endeavour, to include a new class of fundamental ideas in the elementary discipline of the human intellect. This is indispensable, if we wish to educe the powers which we know that it possesses, and to enrich it with the wealth which lies within its reach\*.

8. By the view which is thus presented to us of the nature and objects of intellectual education, we are led to consider the mind of man as undergoing a progress from age to age. By the discoveries which are made,

\* The University of Cambridge has, by a recent law, made an examination in Elementary Mechanics requisite for the Degree of B. A.

and by the clearness and evidence which, after a time, (not suddenly nor soon,) the truths thus discovered acquire, one portion of knowledge after another becomes elementary; and if we would really secure this progress, and make men share in it, these new portions must be treated as elementary in the constitution of a liberal education. Even in the rudest forms of intelligence, man is immeasurably elevated above the unprogressive brute, for the idea of number is so far developed that he can count his flock or his arrows. But when number is contemplated in a speculative form, he has made a vast additional progress; when he steadily apprehends the relations of space, he has again advanced; when in thought he carries these relations into the vault of the sky, into the expanse of the universe, he reaches a higher intellectual position. And when he carries into these wide regions, not only the relations of space and time, but of cause and effect, of force and reaction, he has again made an intellectual advance; which, wide as it is at first, is accessible to all; and with which all should acquaint themselves, if they really desire to prosecute with energy the ascending path of truth and knowledge which lies before them. This should be an object of exertion to all ingenuous and hopeful minds. For, that exertion is necessary,—that after all possible facilities have been afforded, it is still a matter of toil and struggle to appropriate to ourselves the acquisitions of great discoverers, is not to be denied. Elementary mechanics, like elementary geometry, is a study accessible to all: but like that too, or perhaps more than that, it is a study which requires effort and contention of mind,—a forced steadiness of thought. It is long since one complained of this labour in geometry; and was answered that in that region there is no *Royal Road*. The same is true of Mechanics, and must be true of all branches of solid



education. But we should express the truth more appropriately in our days by saying that there is no *Popular Road* to these sciences. In the mind, as in the body, strenuous exercise alone can give strength and activity. The art of exact thought can be acquired only by the labour of close thinking.

9. (IV.) *Chemical Ideas*.—We appear then to have arrived at a point of human progress in which a liberal education of the scientific intellect should include, besides arithmetic, elementary geometry and mechanics. The question then occurs to us, whether there are any other Fundamental Ideas, among those belonging to other sciences, which ought also to be made part of such an education;—whether, for example, we should strive to develop in the minds of all cultured men the ideas of *polarity*, mechanical and chemical, of which we spoke in a former part of this work.

The views to which we have been conducted by the previous inquiry lead us to reply that it would not be well at present to make *chemical Polarities*, at any rate, a subject of elementary instruction. For even the most profound and acute philosophers who have speculated upon this subject,—they who are leading the van in the march of discovery,—do not seem yet to have reduced their thoughts on this subject to a consistency, or to have taken hold of this idea of Polarity in a manner quite satisfactory to their own minds. This part of the subject is, therefore, by no means ready to be introduced into a course of general elementary education; for, with a view to such a purpose, nothing less than the most thoroughly luminous and transparent condition of the idea will suffice. Its whole efficacy, as a means and object of disciplinary study, depends upon there being no obscurity, perplexity, or indefiniteness with regard to it, beyond that transient deficiency which at first exists

in the learner's mind, and is to be removed by his studies. The idea of chemical Polarity is not yet in this condition; and therefore is not yet fit for a place in education. Yet since this idea of Polarity is the most general idea which enters into chemistry, and appears to be that which includes almost all the others, it would be unphilosophical, and inconsistent with all sound views of science, to introduce into education some chemical conceptions, and to omit those which depend upon this idea: indeed such a partial adoption of the science could hardly take place without not only omitting, but misrepresenting, a great part of our chemical knowledge. The conclusion to which we are necessarily led, therefore, is this:—that at present chemistry cannot with any advantage, form a portion of the general intellectual education\*.

10. (V.) *Natural-History Ideas*.—But there remains still another class of Ideas, with regard to which we may very properly ask whether they may not advantageously form a portion of a liberal education: I mean the Ideas of definite Resemblance and Difference, and of one set of resemblances subordinate to another, which form the bases of the classificatory sciences. These Ideas are developed by the study of the various branches of Natural History, as Botany, and Zoology; and beyond all doubt, those pursuits, if assiduously followed, very materially affect the mental habits. There is this obvious advantage to be looked for from the study of Natural History, considered as a means of intellectual disci-

\* I do not here stop to prove that an education (if it be so called) in which the memory only retains the verbal expression of results, while the mind does not apprehend the principles of the subject, and therefore cannot even understand the words in which its doctrines are expressed, is of no value whatever to the intellect, but rather, is highly hurtful to the habits of thinking and reasoning.

pline:—that it gives us, in a precise and scientific form, examples of the classing and naming of objects; which operations the use of common language leads us constantly to perform in a loose and inexact way. In the usual habits of our minds and tongues, things are distinguished or brought together, and names are applied, in a manner very indefinite, vacillating, and seemingly capricious: and we may naturally be led to doubt whether such defects can be avoided;—whether exact distinctions of things, and rigorous use of words be possible. Now upon this point we may receive the instruction of Natural History; which proves to us, by the actual performance of the task, that a precise classification and nomenclature are attainable, at least for a mass of objects all of the same kind. Further, we also learn from this study, that there may exist, not only an exact distinction of kinds of things, but a series of distinctions, one set subordinate to another, and the more general including the more special, so as to form a system of classification. All these are valuable lessons. If by the study of Natural History we evolve, in a clear and well defined form, the conceptions of *genus*, *species*, and of *higher* and *lower steps* of classification, we communicate precision, clearness, and method to the intellect, through a great range of its operations.

11. It must be observed, that in order to attain the disciplinal benefit which the study of Natural History is fitted to bestow, we must teach the *natural* not the artificial *classifications*; or at least the natural as well as the artificial. For it is important for the student to perceive that there are classifications, not merely arbitrary, founded upon some *assumed* character, but natural, recognized by some *discovered* character; he ought to see that our classes being collected according to one mark, are confirmed by many marks not originally stated

in our scheme; and are thus found to be grouped together, not by a single resemblance, but by a mass of resemblances, indicating a natural affinity. That objects may be collected into such groups, is a highly important lesson, which Natural History alone, pursued as the science of *natural classes*, can teach.

12. Natural History has not unfrequently been made a portion of education: and has in some degree produced such effects as we have pointed out. It would appear, however, that its lessons have, for the most part been very imperfectly learnt or understood by persons of ordinary education: and that there are perverse intellectual habits very commonly prevalent in the cultivated classes, which ought ere now to have been corrected by the general teaching of Natural History. We may detect among speculative men many prejudices respecting the nature and rules of reasoning, which arise from pure mathematics having been so long and so universally the instrument of intellectual cultivation. Pure Mathematics reasons from definitions: whatever term is introduced into her pages, as a *circle*, or a *square*, its definition comes along with it: and this definition is supposed to supply all that the reasoner needs to know, respecting the term. If there be any doubt concerning the validity of the conclusion, the doubt is resolved by recurring to the definitions. Hence it has come to pass that in other subjects also, men seek for and demand definitions as the most secure foundation of reasoning. The definition and the term defined are conceived to be so far identical, that in all cases the one may be substituted for the other; and such a substitution is held to be the best mode of detecting fallacies.

13. It has already been shown that even geometry is not founded upon definitions alone: and we shall not here again analyse the fallacy of this belief in the supreme

value of definitions. But we may remark that the study of Natural History appears to be the proper remedy for this erroneous habit of thought. For in every department of Natural History the object of our study is *kinds* of things, not one of which kinds can be rigorously defined, yet all of them are sufficiently definite. In these cases we may indeed give a specific description of one of the kinds, and may call it a definition; but it is clear that such a definition does not contain the essence of the thing. We say\* that the Rose Tribe are "Polypetalous dicotyledons, with lateral styles, superior simple ovaria, regular perigynous stamens, exalbuminous definite seeds, and alternate stipulate leaves." But no one would say that this was our essential conception of a rose, to be substituted for it in all cases of doubt or obscurity, by way of making our reasonings perfectly clear. Not only so; but as we have already seen†, the definition does not even apply to all the tribe. For the stipulæ are absent in *Lowea*: the albumen is present in *Neillia*: the fruit of *Spiræa sorbifolia* is capsular. If, then, we can possess any certain knowledge in Natural History, (which no cultivator of the subject will doubt,) it is evident that our knowledge cannot depend on the possibility of laying down exact definitions and reasoning from them.

14. But it may be asked, if we cannot define a word, or a class of things which a word denotes, how can we distinguish what it does mean from what it does not mean? How can we say that it signifies one thing rather than another, except we declare what is its signification?

The answer to this question involves the general principle of a natural method of classification, which has already been stated‡ and need not here be again dwelt

\* Lindley's *Nat. Syst. Bot.*, p. 81.

† B. VIII., c. ii. sect. 3.

‡ B. VIII., c. ii. *ibid.*

on. It has been shown that names of *kinds* of things (*genera*) associate them according to total resemblances, not partial characters. The principle which connects a group of objects in natural history is not a *definition*, but a *type*. Thus we take as the type of the Rose family, it may be, the common wild rose; all species which resemble this more than they resemble any other group of species are also roses, and form one genus. All genera which resemble Roses more than they resemble any other group of genera are of the same family. And thus the Rose family is collected about some one species, which is the type or central point of the group.

In such an arrangement, it may readily be conceived that though the nucleus of each group may cohere firmly together, the outskirts of contiguous groups may approach, and may even be intermingled, so that some species may doubtfully adhere to one group or another. Yet this uncertainty does not at all affect the truths which we find ourselves enabled to assert with regard to the general mass of each group. And thus we are taught that there may be very important differences between two groups of objects, although we are unable to tell where the one group ends and where the other begins; and that there may be propositions of indisputable truth, in which it is impossible to give unexceptionable definitions of the terms employed.

15. These lessons are of the highest value with regard to all employments of the human mind; for the mode in which words in common use acquire their meaning, approaches far more nearly to the *Method of Type* than to the method of definition. The terms which belong to our practical concerns, or to our spontaneous and unscientific speculations, are rarely capable of exact definition. They have been devised in order to express assertions, often very important, yet very vaguely con-

ceived : and the signification of the word is extended, as far as the assertion conveyed by it can be extended, by apparent connexion or by analogy. And thus, in all the attempts of man to grasp at knowledge, we have an exemplification of that which we have stated as the rule of induction, that Definition and Proposition are mutually dependent, each adjusted so as to give value and meaning to the other : and this is so; even when both the elements of truth are defective in precision : the Definition being replaced by an incomplete description or a loose reference to a Type ; and the Proposition being in a corresponding degree insecure.

16. Thus the study of Natural History, as a corrective of the belief that definitions are essential to substantial truth, might be of great use ; and the advantage which might thus be obtained is such as well entitles this study to a place in a liberal education. We may further observe, that in order that Natural History may produce such an effect, it must be studied by inspection of the *objects* themselves, and not by the reading of books only. Its lesson is, that we must in all cases of doubt or obscurity refer, not to words or definitions, but to things. The Book of Nature is its dictionary : it is there that the natural historian looks, to find the meaning of the words which he uses\*. So long as a plant, in its most essential parts, is more like a rose than anything else, it is a rose. He knows no other definition.

\* It is a curious example of the influence of the belief in definitions, that elementary books have been written in which Natural History is taught in the way of question and answer, and consequently by means of words alone. In such a scheme, of course all objects are *defined* : and we may easily anticipate the value of the knowledge thus conveyed. Thus, "Iron is a well-known hard metal, of a darkish gray colour, and very elastic:" "Copper is an orange-coloured metal, more sonorous than any other, and the most elastic of any except iron." This is to pervert the meaning of education, and to make it a business of mere words.

17. (VI.) *Well-established Ideas alone to be used.*—

We may assert in general what we have above stated specially with reference to the fundamental principles of chemistry:—no Ideas are suited to become the elements of elementary education, till they have not only become perfectly distinct and fixed in the minds of the leading cultivators of the science to which they belong; but till they have been so for some considerable period. The entire clearness and steadiness of view which is essential to sound science, must have time to extend itself to a wide circle of disciples. The views and principles which are detected by the most profound and acute philosophers, are soon appropriated by all the most intelligent and active minds of their own and of the following generations; and when this has taken place, (and not till then,) it is right, by a proper constitution of our liberal education, to extend a general knowledge of such principles to all cultivated persons. And it follows, from this view of the matter, that we are by no means to be in haste to adopt, into our course of education, all new discoveries as soon as they are made. They require some time, in order to settle into their proper place and position in men's minds, and to show themselves under their true aspects; and till this is done, we confuse and disturb, rather than enlighten and unfold, the ideas of learners, by introducing the discoveries into our elementary instruction. Hence it was perhaps reasonable that a century should elapse from the time of Galileo before the rigorous teaching of mechanics became a general element of intellectual training; and the doctrine of universal gravitation was hardly ripe for such an employment till the end of the last century. We must not direct the unformed youthful mind to launch its little bark upon the waters of speculation, till all the agitation



of discovery, with its consequent fluctuation and controversy, has well subsided.

18. But it may be asked, How is it that time operates to give distinctness and evidence to scientific ideas? In what way does it happen that views and principles, obscure and wavering at first, after a while become luminous and steady? Can we point out any process, any intermediate steps, by which this result is produced? If we can, this process must be an important portion of the subject now under our consideration.

To this we reply, that the transition from the hesitation and contradiction with which true ideas are first received, to the general assent and clear apprehension which they afterwards obtain, takes place through various arguments for and against them, and various modes of presenting and testing them, all which we may include under the term Discussion, which we have already mentioned as the second of the two ways by which scientific views are developed into full maturity.

#### CHAPTER IV.

#### OF METHODS OF ACQUIRING CLEAR SCIENTIFIC IDEAS, *continued*.—OF THE DISCUSSION OF IDEAS.

1. It is easily seen that in every part of science, the establishment of a new set of ideas has been accompanied with much of doubt and dissent. And by means of discussions so occasioned, the new conceptions, and the opinions which involve them, have gradually become definite and clear. The authors and asserters of the new opinions, in order to make them defensible, have been compelled to make them consistent: in order to recommend them to others, they have been obliged to make

them more entirely intelligible to themselves. And thus the terms which formed the main points of the controversy, although applied in a loose and vacillating manner at first, have in the end become perfectly definite and exact. The opinions discussed have been, in their main features, the same throughout the debate; but they have at first been dimly, and at last clearly apprehended: like the objects of a landscape, at which we look through a telescope ill adjusted, till, by sliding the tube backwards and forwards, we at last bring it into focus, and perceive every feature of the prospect sharp and bright.

2. We have in the last Book but one\* fully exemplified this gradual progress of conceptions from obscurity to clearness by means of Discussion. We have seen, too, that this mode of treating the subject has never been successful, except when it has been associated with an appeal to facts as well as to reasonings. A combination of experiment with argument, of observation with demonstration, has always been found requisite in order that men should arrive at those distinct conceptions which give them substantial truths. The arguments used led to the rejection of undefined, ambiguous, self-contradictory notions; but the reference to facts led to the selection, or at least to the retention, of the conceptions which were both true and useful. The two correlative processes, definition and true assertion, the formation of clear ideas and the induction of laws, went on together.

Thus those discussions by which scientific conceptions are rendered ultimately quite distinct and fixed, include both reasonings from principles and illustrations from facts. At present we turn our attention more peculiarly to the former part of the process; according to the distinction already drawn, between the explication of conceptions and the colligation of facts. The Discussions

\* B. XI. c. ii. Of the Explication of Conceptions.

of which we here speak, are the Method (if they may be called a *method*) by which the Explication of Conceptions is carried to the requisite point among philosophers.

3. In the scrutiny of the Fundamental Ideas of the Sciences which forms the previous Part of this work, and in the *History of the Inductive Sciences*, I have, in several instances, traced the steps by which, historically speaking, these Ideas have obtained their ultimate and permanent place in the minds of speculative men. I have thus exemplified the reasonings and controversies which constitute such Discussion as we now speak of. I have stated, at considerable length, the various attempts, failures, and advances, by which the ideas which enter into the science of Mechanics were evolved into their present evidence. In like manner we have seen the conception of refracted rays of light, obscure and confused in Seneca, growing clearer in Roger Bacon, more definite in Descartes, perfectly distinct in Newton. The polarity of light, at first contemplated with some perplexity, became very distinct to Malus, Young, and Fresnel; yet the phenomena of circular polarization, and still more, the circular polarization of fluids, leave us, even at present, some difficulty in fully mastering this conception. The related polarities of electricity and magnetism are not yet fully comprehended, even by our greatest philosophers. One of Mr. Faraday's late papers (the Fourteenth Series of his Researches) is employed in an experimental discussion of this subject, which leads to no satisfactory result. The controversy between Biot and Ampère\*, on the nature of the elementary forces in electro-dynamic action, is another evidence that the discussion of this subject has not yet reached its termination. With regard to chemical polarity, I have already stated that this idea is as yet very far from being brought to an ultimate con-

\* *Hist. Ind. Sci.*, B. XIII. c. vi.

dition of definiteness; and the subject of chemical forces, (for the whole subject must be included in this idea of polarity,) which has already occasioned much perplexity and controversy, may easily occasion much more, before it is settled to the satisfaction of the philosophical world. The ideas of the classificatory sciences also have of late been undergoing much, and very instructive discussion, in the controversies respecting the relations and offices of the natural and artificial methods. And with regard to physiological ideas, it would hardly be too much to say, that the whole history of physiology up to the present time has consisted of the discussion of the fundamental ideas of the science, such as vital forces, nutrition, reproduction, and the like. We have had before us at some length, in the present work, a review of the opposite opinions which have been advanced on this subject; and have attempted in some degree to estimate the direction in which these ideas are permanently settling. But without attaching any importance to this attempt, the account there given may at least serve to show, how important a share in the past progress of this subject the discussion of its fundamental ideas has hitherto had.

4. There is one reflection which is very pointedly suggested by what has been said. The manner in which our scientific ideas acquire their distinct and ultimate form being such as has been described,—always involving much abstract reasoning and analysis of our conceptions, often much opposite argumentation and debate;—how unphilosophical is it to speak of abstraction and analysis, of dispute and controversy, as frivolous and unprofitable processes, by which true science can never be benefitted; and to put such employments in antithesis with the study of facts!

Yet some writers are accustomed to talk with contempt of all past controversies, and to wonder at the blind-

ness of those who did not at first take the view which was established at last. Such persons forget that it was precisely the controversy, which established among speculative men that final doctrine which they themselves have quietly accepted. It is true, they have had no difficulty in thoroughly adopting the truth; but that has occurred because all dissentient doctrines have been suppressed and forgotten; and because systems, and books, and language itself, have been accommodated peculiarly to the expression of the accepted truth. To despise those who have, by their mental struggles and conflicts, brought the subject into a condition in which error is almost out of our reach, is to be ungrateful exactly in proportion to the amount of the benefit received. It is as if a child, when its teacher had with many trials and much trouble prepared a telescope so that the vision through it was distinct, should wonder at his stupidity in pushing the tube of the eye-glass out and in so often.

5. Again, some persons condemn all that we have here spoken of as the discussion of ideas, terming it *metaphysical*: and in this spirit, one writer\* has spoken of the "metaphysical period" of each science, as preceding the period of "positive knowledge." But as we have seen, that process which is here termed "metaphysical,"—the analysis of our conceptions and the exposure of their inconsistencies,—(accompanied with the study of facts,)—has always gone on most actively in the most prosperous periods of each science. There is, in Galileo, Kepler, Gassendi, and the other fathers of mechanical philosophy, as much of metaphysics as in their adversaries. The main difference is, that the metaphysics is of a better kind; it is more conformable to metaphysical truth. And the same is the case in other sciences. Nor can it be otherwise. For all truth, before it can be consistent

\* M. Auguste Comte, *Cours de Philosophie Positive*.

with facts, must be consistent with itself: and although this rule is of undeniable authority, its application is often far from easy. The perplexities and ambiguities which arise from our having the same idea presented to us under different aspects, are often difficult to disentangle: and no common acuteness and steadiness of thought must be expended on the task. It would be easy to adduce, from the works of all great discoverers, passages more profoundly metaphysical than any which are to be found in the pages of barren *à priori* reasoners.

6. As we have said, these metaphysical discussions are not to be put in opposition to the study of facts; but are to be stimulated, nourished and directed by a constant recourse to experiment and observation. The cultivation of ideas is to be conducted as having for its object the connexion of facts; never to be pursued as a mere exercise of the subtilty of the mind, striving to build up a world of its own, and neglecting that which exists about us. For although man may in this way please himself, and admire the creations of his own brain, he can never, by this course, hit upon the real scheme of nature. With his ideas unfolded by education, sharpened by controversy, rectified by metaphysics, he may *understand* the natural world, but he cannot *invent* it. At every step, he must try the value of the advances he has made in thought, by applying his thoughts to things. The Explication of Conceptions must be carried on with a perpetual reference to the Colligation of Facts.

Having here treated of Education and Discussion as the methods by which the former of these two processes is to be promoted, we have now to explain the methods which science employs in order most successfully to execute the latter. But the Colligation of Facts, as already stated, may offer to us two steps of a very different kind,—the laws of Phenomena, and their Causes. We

shall first describe some of the methods employed in obtaining truths of the former of these two kinds.

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## CHAPTER V.

### ANALYSIS OF THE PROCESS OF INDUCTION.

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#### SECT. I.—*The Three Steps of Induction.*

1. WHEN facts have been decomposed and phenomena measured, the philosopher endeavours to combine them into general laws, by the aid of ideas and conceptions, these being illustrated and regulated by such means as we have spoken of in the last two chapters. In this task, of gathering laws of nature from observed facts, as we have already said\*, the natural sagacity of gifted minds is the power by which the greater part of the successful results have been obtained; and this power will probably always be more efficacious than any Method can be. Still there are certain methods of procedure which may in such investigations give us no inconsiderable aid, and these I shall endeavour to expound.

2. For this purpose, I remark that the Colligation of ascertained facts into general propositions may be considered as containing three steps, which I shall term *the Selection of the Idea, the Construction of the Conception, and the Determination of the Magnitudes*. It will be recollected that by the word *Idea*, (or Fundamental Idea,) used in a peculiar sense, I mean certain wide and general fields of intelligible relation, such as Space, Number, Cause, Likeness; while by *Conception* I denote more special modifications of these ideas, as a *circle*, a *square number*, a *uniform force*, a *like form of flower*. Now in

\* B. xi. c. vi.

order to establish any law by reference to facts, we must select the *true Idea* and the *true Conception*. For example; when Hipparchus found\* that the distance of the bright star Spica Virginis from the equinoxial point had increased by two degrees in about two hundred years, and desired to reduce this change to a law, he had first to assign, if possible, the *idea* on which it depended;—whether it was regulated for instance, by *space*, or by *time*; whether it was determined by the positions of other stars at each moment, or went on progressively with the lapse of ages. And when there was found reason to select *time* as the regulative *idea* of this change, it was then to be determined how the change went on with the time;—whether uniformly, or in some other manner: the *conception*, or the rule of the progression, was to be rightly constructed. Finally, it being ascertained that the change did go on uniformly, the question then occurred what was its *amount*:—whether exactly a degree in a century, or more, or less, and how much: and thus the determination of the *magnitude* completed the discovery of the law of phenomena respecting this star.

3. Steps similar to these three may be discerned in all other discoveries of laws of nature. Thus, in investigating the laws of the motions of the sun, moon or planets, we find that these motions may be resolved, besides a uniform motion, into a series of partial motions, or Inequalities; and for each of these Inequalities, we have to learn upon what it directly depends, whether upon the progress of time only, or upon some configuration of the heavenly bodies in space; then, we have to ascertain its law; and finally, we have to determine what is its amount. In the case of such Inequalities, the fundamental element on which the Inequality depends, is called the *Argument*. And when the Inequality has been

\* *Hist. Ind. Sci.*, B. III. c. iv. sect. 3.



fully reduced to known rules, and expressed in the form of a Table, the Argument is the fundamental series of numbers which stands in the margin of the Table, and by means of which we refer to the other numbers which express the Inequality. Thus, in order to obtain from a Solar Table the Inequality of the sun's annual motion, the Argument is the number which expresses the day of the year; the Inequalities for each day being (in the Table) ranged in a line corresponding to the days. Moreover, the Argument of an Inequality being assumed to be known, we must, in order to calculate the Table, that is, in order to exhibit the law of nature, know also the *Law* of the Inequality, and its *Amount*. And the investigation of these three things, the Argument, the Law, and the Amount of the Inequality, represents the three steps above described, the Selection of the Idea, the Construction of the Conception, and the Determination of the Magnitude.

4. In a great body of cases, *mathematical* language and calculation are used to express the connexion between the general law and the special facts. And when this is done, the three steps above described may be spoken of as the Selection of the *Independent Variable*, the Construction of the *Formula*, and the Determination of the *Coefficients*. It may be worth our while to attend to an exemplification of this. Suppose then, that, in such observations as we have just spoken of, namely, the shifting of a star from its place in the heavens by an unknown law, astronomers had, at the end of three successive years, found that the star had removed by 3, by 8, and by 15 minutes from its original place. Suppose it to be ascertained also, by methods of which we shall hereafter treat, that this change depends upon the time; we must then take the *time*, (which we may denote by the symbol  $t$ ,) for the *independent variable*. But though the star changes

its place with the time, the change is not proportional to the time; for its motion which is only 3 minutes in the first year, is 5 minutes in the second year, and 7 in the third. But it is not difficult for a person a little versed in mathematics to perceive that the series 3, 8, 15, may be obtained by means of two terms, one of which is proportional to the time, and the other to the square of the time; that is, it is expressed by the *formula*  $at + btt$ . The question then occurs, what are the values of the *coefficients*  $a$  and  $b$ ; and a little examination of the case shows us that  $a$  must be 2, and  $b$ , 1: so that the formula is  $2t + tt$ . Indeed if we add together the series 2, 4, 6, which expresses the change proportional to the time, and 1, 4, 9, which is proportional to the square of the time, we obtain the series 3, 8, 15, which is the series of numbers given by observation. And thus the three steps which give us the Idea, the Conception, and the Magnitudes; or the Argument, the Law, and the Amount, of the change; give us the Independent Variable, the Formula, and the Coefficients, respectively.

We now proceed to offer some suggestions of methods by which each of these steps may be in some degree promoted.

## SECT. II.—*Of the Selection of the Fundamental Idea.*

5. WHEN we turn our thoughts upon any assemblage of facts, with a view of collecting from them some connexion or law, the most important step, and at the same time that in which rules can least aid us, is the Selection of the Idea by which they are to be collected. So long as this idea has not been detected, all seems to be hopeless confusion or insulated facts; when the connecting idea has been caught sight of, we constantly regard the facts with reference to their connexion, and wonder that

it should be possible for any one to consider them in any other point of view.

Thus the different seasons, and the various aspects of the heavenly bodies, might at first appear to be direct manifestations from some superior power, which man could not even understand: but it was soon found that the ideas of time and space, of motion and recurrence, would give coherency to many of the phenomena. Yet this took place by successive steps. Eclipses, for a long period, seemed to follow no law; and being very remarkable events, continued to be deemed the indications of a supernatural will, after the common motions of the heavens were seen to be governed by relations of time and space. At length, however, the Chaldeans discovered that, after a period of eighteen years, similar sets of eclipses recur; and, thus selecting the idea of *time*, simply, as that to which these events were to be referred, they were able to reduce them to rule; and from that time, eclipses were recognized as parts of a regular order of things. We may, in the same manner, consider any other course of events, and may enquire by what idea they are bound together. For example, if we take the weather, years peculiarly wet or dry, hot and cold, productive and unproductive, follow each other in a manner which, at first sight at least, seems utterly lawless and irregular. Now can we in any way discover some rule and order in these occurrences? Is there, for example, in these events, as in eclipses, a certain cycle of years, after which like seasons come round again? or does the weather depend upon the force of some extraneous body—for instance, the moon—and follow in some way her aspects? or would the most proper way of investigating this subject be to consider the effect of the moisture and heat of various tracts of the earth's surface upon the ambient air? It is at our choice to *try* these and other modes of

obtaining a science of the weather : that is, we may refer the phenomena to the idea of *time*, introducing the conception of a cycle ;—or to the idea of external *force*, by the conception of the moon's action ;—or to the idea of *mutual action*, introducing the conceptions of thermo-tical and atmological agencies, operating between different regions of earth, water, and air.

6. It may be asked, How are we to decide in such alternatives ? How are we to select the one right idea out of several conceivable ones ? To which we can only reply, that this must be done by *trying* which will succeed. If there really exist a cycle of the weather, as well as of eclipses, it must be established by comparing the asserted cycle with a good register of the seasons, of sufficient extent. Or if the moon really influence the meteorological conditions of the air, the asserted influence must be compared with the observed facts, and so accepted or rejected. When Hipparchus had observed the increase of longitude of the stars, the idea of a motion of the celestial sphere suggested itself as the explanation of the change ; but this thought was verified only by observing several stars. It was conceivable that each star should have an independent motion, governed by time only, or by other circumstances, instead of being regulated by its place in the sphere ; and this possibility could be rejected by trial alone. In like manner, the original opinion of the composition of bodies supposed the compounds to derive their properties from the elements according to the law of *likeness* ; but this opinion was overturned by a thousand facts ; and thus the really applicable idea of chemical composition was introduced in modern times. In what has already been said on the History of Ideas, we have seen how each science was in a state of confusion and darkness till the right idea was introduced.

7. No general method of evolving such ideas can be given. Such events appear to result from a peculiar sagacity and felicity of mind;—never without labour, never without preparation;—yet with no constant dependence upon preparation, or upon labour, or even entirely upon personal endowments. Newton explained the colours which refraction produces, by referring each colour to a peculiar *angle of refraction*, thus introducing the right idea. But when the same philosopher tried to explain the colours produced by diffraction, he erred, by attempting to apply the same idea, (*the course of a single ray*,) instead of applying the truer idea of the *interference of two rays*. Newton gave a wrong rule for the double refraction of Iceland spar, by making the refraction depend on the *edges* of the rhombohedron: Huyghens, more happy, introduced the idea of the *axis of symmetry* of the solid, and thus was able to give the true law of the phenomena.

8. Although the selected idea is proved to be the right one, only when the true law of nature is established by means of it, yet it often happens that there prevails a settled conviction respecting the relation which must afford the key to the phenomena, before the selection has been confirmed by the laws to which it leads. Even before the empirical laws of the tides were made out, it was not doubtful that these laws depended upon the places and motions of the sun and moon. We know that the crystalline form of a body must depend upon its chemical composition, though we are as yet unable to assign the law of this dependence.

Indeed in most cases of great discoveries, the right idea to which the facts were to be referred, was selected by many philosophers, before the decisive demonstration that it *was* the right idea, was given by the discoverer. Thus Newton showed that the motions of the planets

might be explained by means of a central force in the sun : but though he established, he did not first select the idea involved in the conception of a central force. The idea had already been sufficiently pointed out, dimly by Kepler, more clearly by Borelli, Huyghens, Wren, and Hooke. Indeed this anticipation of the true idea is always a principal part of that which, in the *History of the Sciences*, we have termed the *Prelude* of a Discovery. The two steps of *proposing* a philosophical problem, and of *solving* it, are, as we have elsewhere said, both important, and are often performed by different persons. The former step is, in fact, the Selection of the Idea. In explaining any change, we have to discover first the *Argument*, and then the *Law* of the change. The selection of the Argument is the step of which we here speak ; and is that in which inventiveness of mind and justness of thought are mainly shown.

9. Although, as we have said, we can give few precise directions for this cardinal process, the Selection of the Idea, in speculating on phenomena, yet there is one Rule which may have its use : it is this :—*The idea and the facts must be homogeneous* : the elementary Conceptions, into which the facts have been decomposed, must be of the same nature as the Idea by which we attempt to collect them into laws. Thus, if facts have been observed and measured by reference to space, they must be bound together by the idea of space : if we would obtain a knowledge of mechanical forces in the solar system, we must observe mechanical phenomena. Kepler erred against this rule in his attempts at obtaining physical laws of the system ; for the facts which he took were the *velocities*, not the *changes of velocity*, which are really the mechanical facts. Again, there has been a transgression of this Rule committed by all chemical philosophers who have attempted to assign the relative position of the ele-

mentary particles of bodies in their component molecules. For their purpose has been to discover the *relations* of the particles *in space*; and yet they have neglected the only facts in the constitution of bodies which have a reference to space—namely, *crystalline form*, and *optical properties*. No progress can be made in the theory of the elementary structure of bodies, without making these classes of facts the main basis of our speculations.

10. The only other Rule which I have to offer on this subject, is that which I have already given:—*the Idea must be tested by the facts*. It must be tried by applying to the facts the conceptions which are derived from the idea, and not accepted till some of these succeed in giving the law of the phenomena. The justice of the suggestion cannot be known otherwise than by making the trial. If we can discover a *true law* by employing any conceptions, the idea from which these conceptions are derived is the *right* one; nor can there be any proof of its rightness so complete and satisfactory, as that we are by it led to a solid and permanent truth.

This, however, can hardly be termed a Rule; for when we would know, to conjecture and to try the truth of our conjecture by a comparison with the facts, is the natural and obvious dictate of common sense.

Supposing the Idea which we adopt, or which we would try, to be now fixed upon, we still have before us the range of many Conceptions derived from it; many Formulæ may be devised depending on the same Independent Variable, and we must now consider how our selection among these is to be made.

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## CHAPTER VI.

### GENERAL RULES FOR THE CONSTRUCTION OF THE CONCEPTION.

1. In speaking of the discovery of laws of nature, those which depend upon *quantity*, as number, space, and the like, are most prominent and most easily conceived, and therefore in speaking of such researches, we shall often use language which applies peculiarly to the cases in which quantities numerically measurable are concerned, leaving it for a subsequent task to extend our principles to ideas of other kinds.

Hence we may at present consider the Construction of a Conception which shall include and connect the facts, as being the construction of a Mathematical Formula, coinciding with the numerical expression of the facts; and we have to consider how this process can be facilitated, it being supposed that we have already before us the numerical measures given by observation.

2. We may remark, however, that the construction of the right Formula for any such case, and the determination of the Coefficients of such formula, which we have spoken of as two separate steps, are in practice almost necessarily simultaneous; for the near coincidence of the results of the theoretical rule with the observed facts confirms at the same time the Formula and its Coefficients. In this case also, the mode of arriving at truth is to try various hypotheses;—to modify the hypotheses so as to approximate to the facts, and to multiply the facts so as to test the hypotheses.

The Independent Variable, and the Formula which we would try, being once selected, mathematicians have devised certain special and technical processes by which the value of the coefficients may be determined. These



we shall treat of in the next Chapter; but in the mean time we may note, in a more general manner, the mode in which, in physical researches, the proper formula may be obtained.

3. A person somewhat versed in mathematics, having before him a series of numbers, will generally be able to devise a formula which approaches near to those numbers. If, for instance, the series is constantly progressive, he will be able to see whether it more nearly resembles an arithmetical or a geometrical progression. For example, MM. Dulong and Petit, in their investigation of the law of cooling of bodies, obtained the following series of measures. A thermometer, made hot, was placed in an enclosure of which the temperature was 0 degrees, and the rapidity of cooling of the thermometer was noted for many temperatures. It was found that

|                     |     |                             |       |
|---------------------|-----|-----------------------------|-------|
| For the temperature | 240 | the rapidity of cooling was | 10.69 |
| "                   | 220 | "                           | 8.81  |
| "                   | 200 | "                           | 7.40  |
| "                   | 180 | "                           | 6.10  |
| "                   | 160 | "                           | 4.89  |
| "                   | 140 | "                           | 3.88  |

and so on. Now this series of numbers manifestly increases with greater rapidity as we proceed from the lower to the higher parts of the scale. The numbers do not, however, form a geometrical series, as we may easily ascertain. But if we were to take the differences of the successive terms we should find them to be—

1.88, 1.41, 1.30, 1.21, 1.01, &c.

and these numbers are very nearly the terms of a geometric series. For if we divide each term by the succeeding one, we find these numbers,

1.33, 1.09, 1.07, 1.20, 1.27,

in which there does not appear to be any constant tendency to diminish or increase. And we shall find that a

geometrical series in which the ratio is 1·165, may be made to approach very near to this series, the deviations from it being only such as may be accounted for by conceiving them as errors of observation. In this manner a certain formula\* is obtained, giving results which very nearly coincide with the observed facts, as may be seen in the margin.

The physical law expressed by the formula just spoken of is this:—that when a body is cooling in an empty inclosure at a constant temperature, the quickness of the cooling, for excesses of temperature in arithmetical progression, increases as the terms of a geometrical progression, diminished by a constant number.

4. In the actual investigation of Dulong and Petit, however, the formula was not obtained in precisely the manner just described. For the quickness of cooling depends upon two elements, the temperature of the hot body and the temperature of the inclosure; not merely upon the *excess* of one of these over the other. And it was found most convenient, first, to make such experiments as should exhibit the dependence of the velocity

\* The formula is  $v = 2,037 (a^t - 1)$  where  $v$  is the velocity of cooling,  $t$  the temperature of the thermometer expressed in degrees, and  $a$  is the quantity 1,0077.

The degree of coincidence is as follows:—

| Excess of temperature of<br>the thermometer, or<br>values of $t$ . | Observed<br>values<br>of $v$ . | Calculated<br>values<br>of $v$ . |
|--------------------------------------------------------------------|--------------------------------|----------------------------------|
| 240 . .                                                            | 10·69 . .                      | 10·68                            |
| 220 . .                                                            | 8·81 . .                       | 8·89                             |
| 200 . .                                                            | 7·40 . .                       | 7·34                             |
| 180 . .                                                            | 6·10 . .                       | 6·03                             |
| 160 . .                                                            | 4·89 . .                       | 4·87                             |
| 140 . .                                                            | 3·88 . .                       | 3·89                             |
| 120 . .                                                            | 3·02 . .                       | 3·05                             |
| 100 . .                                                            | 2·30 . .                       | 2·33                             |
| 80 . .                                                             | 1·74 . .                       | 1·72                             |

of cooling upon the temperature of the enclosure; which dependence is contained in the following law:—The quickness of cooling of a thermometer in vacuo for a constant excess of temperature, increases in geometric progression, when the temperature of the inclosure increases in arithmetic progression. From this law the preceding one follows by necessary consequence\*.

This example may serve to show the nature of the artifices which may be used for the construction of formulæ, when we have a constantly progressive series of numbers to represent. We must not only endeavour by trial to contrive a formula which will answer the conditions, but we must vary our experiments so as to determine, first one factor or portion of the formula, and then the other; and we must use the most probable hypothesis as means of suggestion for our formulæ.

5. In a *progressive* series of numbers, except the formula which we adopt be really that which expresses the law of nature; the deviations of the formula from the facts will generally become enormous, when the experiments are extended into new parts of the scale. True formulæ for a progressive series of results can hardly ever be obtained from a very limited range of experiments: just, as the attempt to guess the general course of a road or a river, by knowing two or three points of it in the neighbourhood of one another, would generally fail. In the investigation respecting the laws of the

\* For if  $\theta$  be the temperature of the inclosure, and  $t$  the excess of temperature of the hot body, it appears, by this law, that the radiation of heat is as  $a^\theta$ . And hence the quickness of cooling, which is as the excess of radiation, is as  $a^{\theta+t} - a^\theta$ ; that is, as  $a^\theta (a^t - 1)$  which agrees with the formula given in the last note.

The whole of this series of researches of Dulong and Petit is full of the most beautiful and instructive artifices for the construction of the proper formulæ in physical research.

cooling of bodies just noticed, one great advantage of the course pursued by the experimenters was, that their experiments included so great a range of temperatures. The attempts to assign the law of elasticity of steam deduced from experiments made with moderate temperatures, were found to be enormously wrong, when very high temperatures were made the subject of experiment. It is easy to see that this must be so: an arithmetical and a geometrical series may nearly coincide for a few terms moderately near each other: but if we take remote corresponding terms in the two series, one of these will be very many times the other. And hence, from a narrow range of experiments, we may infer one of these series when we ought to infer the other; and thus obtain a law which is widely erroneous.

6. In Astronomy, the serieses of observations which we have to study are, for the most part, not progressive, but *recurrent*. The numbers observed do not go on constantly increasing; but after increasing up to a certain amount they diminish; then, after a certain space, increase again; and so on, changing constantly through certain *cycles*. In cases in which the observed numbers are of this kind, the formula which expresses them must be a *circular function*, of some sort or other; involving, for instance, sines, tangents, and other forms of calculation, which have recurring values when the angle on which they depend goes on constantly increasing. The main business of formal astronomy consists in resolving the celestial phenomena into a series of *terms* of this kind, in detecting their *arguments*, and in determining their *coefficients*.

7. In constructing the formulæ by which laws of nature are expressed, although the first object is to assign the law of the phenomena, philosophers have, in almost all cases, not proceeded in a purely empirical manner, to

connect the observed numbers by some expression of calculation, but have been guided, in the selection of their formula, by some *hypothesis* respecting the mode of connexion of the facts. Thus the formula of Dulong and Petit above given was suggested by the theory of exchanges; the first attempts at the resolution of the heavenly motions into circular functions were clothed in the hypothesis of epicycles. And this was almost inevitable. "We must confess," says Copernicus\*, "that the celestial motions are circular, or compounded of several circles, since their inequalities observe a fixed law, and recur in value at certain intervals, which could not be except they were circular: for a circle alone can make that quantity which has occurred recur again." In like manner the first publication of the *law of the sines*, the true formula of optical refraction, was accompanied by Descartes with an hypothesis, in which an explanation of the law was pretended. In such cases, the mere comparison of observations may long fail in suggesting the true formulæ. The fringes of shadows and other diffracted colours were studied in vain by Newton, Grimaldi, Comparetti, the elder Herschel, and Mr. Brougham, so long as these inquirers attempted merely to trace the laws of the facts as they appeared in themselves; while Young, Fresnel, Fraunhofer, Schwerdt, and others, determined these laws in the most rigorous manner, when they applied to the observations the hypothesis of interferences.

8. But with all the aid that hypotheses and calculation can afford, the construction of true formulæ, in those cardinal discoveries by which the progress of science has mainly been caused, has been a matter of great labour and difficulty, and of good fortune added to sagacity. In the *History of Science*, we have seen how long and how

\* *De Rev.*, L. I. c. iv.

hard Kepler laboured, before he converted the formula for the planetary motions, from an epicyclical combination, to a simple ellipse. The same philosopher, labouring with equal zeal and perseverance to discover the formula of optical refraction, which now appears to us so simple, was utterly foiled. Malus sought in vain the formula determining the angle at which a transparent surface polarizes light: Sir D. Brewster\*, with a happy sagacity, discovered the formula to be simply this, that the index of refraction is the tangent of the angle of polarization.

Though we cannot give rules which will be of much service when we have thus to divine the general form of the relation by which phenomena are connected, there are certain methods by which, in a narrower field, our investigations may be materially promoted;—certain special methods of obtaining laws from observations. Of these we shall now proceed to treat.

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## CHAPTER VII.

### SPECIAL METHODS OF INDUCTION APPLICABLE TO QUANTITY.

IN cases where the phenomena admit of numerical measurement and expression, certain mathematical methods may be employed to facilitate and give accuracy to the determination of the formula by which the observations are connected into laws. Among the most usual and important of these Methods are the following:—

- I. The Method of Curves.
- II. The Method of Means.
- III. The Method of Least Squares.
- IV. The Method of Residues.

\* *Hist. Ind. Sci.*, B. ix. c. vi.

SECT. I.—*The Method of Curves.*

1. THE Method of Curves proceeds upon this basis; that when one quantity undergoes a series of changes depending on the progress of another quantity, (as, for instance, the Deviation of the Moon from her equable place depends upon the progress of Time,) this dependence may be expressed by means of a *curve*. In the language of mathematicians, the variable quantity, whose changes we would consider, is made the *ordinate* of the curve, and the quantity on which the changes depend is made the *abscissa*. In this manner, the curve will exhibit in its form a series of undulations, rising and falling so as to correspond with the alternate increase and diminution of the quantity represented, at intervals of space which correspond to the intervals of time, or other quantity by which the changes are regulated. Thus, to take another example, if we set up, at equal intervals, a series of ordinates representing the height of all the successive high waters brought by the tides at a given place, for a year, the curve which connects the summits of all these ordinates will exhibit a series of undulations, ascending and descending once in about each fortnight; since, in that interval, we have, in succession, the high spring tides and the low neap tides. The curve thus drawn offers to the eye a picture of the order and magnitude of the changes to which the quantity under contemplation, (the height of high water,) is subject.

2. Now the peculiar facility and efficacy of the Method of Curves depends upon this circumstance;—that order and regularity are more readily and clearly recognized, when thus exhibited to the eye in a picture, than they are when presented to the mind in any other manner. To detect the relations of Number considered directly as Number, is not easy: and we might contem-

plate for a long time a Table of recorded Numbers without perceiving the order of their increase and diminution, even if the law were moderately simple; as any one may satisfy himself by looking at a Tide Table. But if these Numbers are expressed by the magnitude of *Lines*, and if these Lines are arranged in regular order, the eye readily discovers the rule of their changes: it follows the curve which runs along their extremities, and takes note of the order in which its convexities and concavities succeed each other, if any order be readily discoverable. The separate observations are in this manner compared and generalized and reduced to rule by the eye alone. And the eye, so employed, detects relations of order and succession with a peculiar celerity and evidence. If, for example, we thus arrange as ordinates the prices of corn in each year for a series of years, we shall see the order, rapidity, and amount of the increase and decrease of price, far more clearly than in any other manner. And if there were any recurrence of increase and decrease at stated intervals of years, we should in this manner perceive it. The eye, constantly active and busy, and employed in making into shapes the hints and traces of form which it contemplates, runs along the curve thus offered to it; and as it travels backwards and forwards, is ever on the watch to detect some resemblance or contrast between one part and another. And these resemblances and contrasts, when discovered, are the images of laws of phenomena; which are made manifest at once by this artifice, although the mind could not easily catch the indications of their existence, if they were not thus reflected to her in the clear mirror of space.

Thus when we have a series of good observations, and know the argument upon which their change of magnitude depends, the Method of Curves enables us to ascer-



once drawn," he says, "must represent, it is evident, the law of variation of the angle of position, with the time, not only for instants intermediate between the dates of observations, but even at the moments of observation themselves, much better than the individual *raw* observations can possibly (on an average) do. It is only requisite to try a case or two, to be satisfied that by substituting the curve for the points, we have made a nearer approach to nature, and in a great measure eliminated errors of observation." "In following the graphical process," he adds, "we have a conviction almost approaching to moral certainty that we cannot be greatly misled." Again, having thus corrected the raw observations, he makes another use of the graphical method, by trying whether an ellipse can be drawn "if not *through*, at least *among* the points, so as to approach tolerably near them all; and thus approaching to the orbit which is the subject of investigation."

5. The *obstacles* which principally impede the application of the method of curves are (I.) our *ignorance of the argument* of the changes, and (II.) the *complication of several laws* with one another.

(I.) If we do not know on what quantity those changes depend which we are studying, we may fail entirely in detecting the law of the changes, although we throw the observations into curves. For the true argument of the change should, in fact, be made the abscissa of the curve. If we were to express, by a series of ordinates, the *hour* of high water on successive days, we should not obtain, or should obtain very imperfectly, the law which these times follow; for the real argument of this change is not the *solar* hour, but the hour at which the *moon* passes the meridian. But if we are supposed to be aware that *this* is the argument, (which theory suggests and trial instantly confirms) we then do immediately obtain the primary

rules of the time of high water, by throwing a series of observations into a curve, with the hour of the moon's transit for the abscissa.

In like manner, when we have obtained the first great or semi-mensual inequality of the tides, if we endeavour to discover the laws of other inequalities by means of curves, we must take from theory the suggestion that the Arguments of such inequalities will probably be the *parallax* and the *declination* of the moon. This suggestion again is confirmed by trial; but if we were supposed to be entirely ignorant of the dependence of the changes of the tide on the distance and declination of the moon, the curves would exhibit unintelligible and seemingly capricious changes. For by the effect of the inequality arising from the parallax, the convexities of the curves which belong to the spring tides, are in some years made alternately greater and less all the year through; while in other years they are made all nearly equal. This difference does not betray its origin, till we refer it to the parallax; and the same difficulty in proceeding would arise if we were ignorant that the moon's declination is one of the arguments of tidal changes.

In like manner, if we try to reduce to law any meteorological changes, those of the height of the barometer for instance, we find that we can make little progress in the investigation, precisely because we do not know the Argument on which these changes depend. That there is a certain regular *diurnal* change of small amount we know; but when we have abstracted this inequality, (of which the Argument is the *time of day*,) we find far greater changes left behind, from day to day and from hour to hour; and we express these in curves, but we cannot reduce them to rule, because we cannot discover on what numerical quantity they depend. The assiduous study of barometrical observations, thrown into curves,

may perhaps hereafter point out to us what are the relations of time and space by which these variations are determined; but in the mean time, this subject exemplifies to us our remark, that the method of curves is of comparatively small use, so long as we are in ignorance of the real Arguments of the Inequalities.

6. (II.) In the next place, I remark that a difficulty is thrown in the way of the method of curves by *the combination of several laws* one with another. It will readily be seen that such a cause will produce a complexity in the curves which exhibit the succession of facts. If, for example, we take the case of the tides, the height of high water increases and diminishes with the approach of the sun to, and its recess from, the syzygies of the moon. Again, this height increases and diminishes as the moon's parallax increases and diminishes; and again, the height diminishes when the declination increases, and *vice versa*; and all these Arguments of change, the distance from syzygy, the parallax, the declination, complete their circuit and return into themselves in different periods. Hence the curve which represents the height of high water has not any periodical interval in which it completes its changes and commences a new cycle. The sinuosity which would arise from each inequality separately considered, interferes with, disguises, and conceals the others; and when we first cast our eyes on the curve of observation, it is very far from offering any obvious regularity in its form. And it is to be observed that we have not yet enumerated *all* the elements of this complexity: for there are changes of the tide depending upon the parallax and declination of the sun as well as of the moon. Again; besides these changes, of which the arguments are obvious, there are others, as those depending upon the barometer and the wind, which follow no known

regular law, and which constantly affect and disturb the results produced by other laws.

In the tides, and in like manner in the motions of the moon, we have very eminent examples of the way in which the discovery of laws may be rendered difficult by the number of laws which operate to affect the same quantity. In such cases, the inequalities are generally picked out in succession, nearly in the order of their magnitudes. In this way there were successively collected, from the study of the moon's motions by a series of astronomers, those Inequalities which we term the *Equation of the Center*, the *Evection*, the *Variation*, and the *Annual Equation*. These Inequalities were not, in fact, obtained by the application of the Method of Curves; but the Method of Curves might have been applied to such a case with great advantage. The Method has been applied with great industry and with remarkable success to the investigation of the laws of the tides; and by the use of it, a series of Inequalities both of the Times and of the Heights of high water has been detected, which explain all the main features of the observed facts.

## SECT. II.—*The Method of Means.*

7. THE Method of Curves, as we have endeavoured to explain above, frees us from the casual and extraneous irregularities which arise from the imperfection of observation; and thus lays bare the results of the laws which really operate, and enables us to proceed in search of those laws. But the Method of Curves is not the only one which effects such a purpose. The errors arising from detached observations may be got rid of, and the additional accuracy which multiplied observations give may be obtained, by operations upon the observed numbers without expressing them by spaces. The process of curves assumes that the errors of observation balance

each other ;—that the accidental excesses and defects are nearly equal in amount ;—that the true quantities which would have been observed if all accidental causes of irregularity were removed, are obtained, exactly or nearly, by selecting quantities, upon the whole, equally distant from the extremes of great and small which our imperfect observations offer to us. But when, among a number of unequal quantities, we take a quantity equally distant from the greater and the smaller, this quantity is termed the *Mean* of the unequal quantities. Hence the correction of our observations by the method of curves consists in taking the Mean of the observations.

8. Now without employing curves, we may proceed arithmetically to take the Mean of all the observed numbers of each class. Thus, if we wished to know the height of the spring tide at a given place, and if we found that four different spring tides were measured as being of the height of ten, thirteen, eleven, and fourteen feet, we should conclude that the true height of the tide was the *Mean* of these numbers,—namely, twelve feet ; and we should suppose that the deviation from this height, in the individual cases, arose from the accidents of weather, the imperfections of observation, or the operation of other laws, besides the alternation of spring and neap tides.

This process of finding the Mean of an assemblage of observed numbers is much practised in discovering, and still more in confirming and correcting, laws of phenomena. We shall notice a few of its peculiarities.

9. The Method of Means requires a knowledge of the *Argument* of the changes which we would study ; for the numbers must be arranged in certain Classes, before we find the Mean of each Class ; and the principle on which this arrangement depends is the *Argument*. This knowledge of the *Argument* is more indispensably neces-

sary in the Method of Means than the Method of Curves; for when curves are drawn, the eye often spontaneously detects the law of recurrence in their sinuosities; but when we have collections of numbers, we must divide them into classes by a selection of our own. Thus, in order to discover the law which the heights of the tide follow, in the progress from spring to neap, we arrange the observed tides according to the *day of the moon's age*; and we then take the mean of all those which thus happen at the same period of the moon's revolution. In this manner we obtain the law which we seek; and the process is very nearly the same in all other applications of this Method of Means. In all cases, we begin by assuming the Classes of measures which we wish to compare, the Law which we could confirm or correct, the Formula of which we would determine the coefficients.

10. The Argument being thus assumed, the Method of Means is very efficacious in ridding our inquiry of errors and irregularities which would impede and perplex it. Irregularities which are altogether accidental, or at least accidental with reference to some law which we have under consideration, compensate each other in a very remarkable way, when we take the means of many observations. If we have before us a collection of observed tides, some of them may be elevated, some depressed by the wind, some noted too high and some too low by the observer, some augmented and some diminished by un contemplated changes in the moon's distance or motion: but in the course of a year or two at the longest, all these causes of irregularity balance each other; and the law of succession, which runs through the observations, comes out as precisely as if those disturbing influences did not exist. In any particular case, there appears to be no possible reason why the deviation should be in one way, or of one moderate amount, rather than

another. But taking the mass of observations together, the deviations in opposite ways will be of equal amount, with a degree of exactness very striking. This is found to be the case in all inquiries where we have to deal with observed numbers upon a large scale. In the progress of the population of a country, for instance, what can appear more inconstant, in detail, than the causes which produce births and deaths? yet in each country, and even in each province of a country, the proportions of the whole numbers of births and deaths remain nearly constant. What can be more seemingly beyond the reach of rule than the occasions which produce letters that cannot find their destination? yet it appears that the number of "dead letters" is nearly the same from year to year. And the same is the result when the deviations arise, not from mere accident, but from laws perfectly regular, though not contemplated in our investigation\*. Thus the effects of the Moon's Parallax upon the Tides, sometimes operating one way and sometimes another, according to certain rules, are quite eliminated by taking the Means of a long series of observations; the excesses and defects neutralizing each other, so far as concerns the effect upon any law of the tides which we would investigate.

11. In order to obtain very great accuracy, very large masses of observations are often employed by philosophers, and the accuracy of the result increases with the multitude of observations. The immense collections of astronomical observations which have in this manner been employed in order to form and correct the tables of the celestial motions are perhaps the most signal instances of the attempts to obtain accuracy by this accumulation of observations. Delambre's Tables of the Sun are

\* Provided the argument of the law which we neglect have no coincidence with the argument of the law which we would determine.

founded upon nearly 3000 observations; Burg's Tables of the Moon upon above 4000.

But there are other instances hardly less remarkable. Mr. Lubbock's first investigations of the laws of the tides of London\*, included above 13,000 observations, extending through nineteen years; it being considered that this large number was necessary to remove the effects of accidental causes†. And the attempts to discover the laws of change in the barometer have led to the performance of labours of equal amount: Laplace and Bouvard examined this question by means of observations made at the Observatory of Paris, four times every day for eight years.

12. We may remark one striking evidence of the accuracy thus obtained by employing large masses of observations. In this way we may often detect inequalities much smaller than the errors by which they are encumbered and concealed. Thus the diurnal oscillations of the barometer were discovered by the comparison of observations of many days, classified according to the hours of the day; and the result was a clear and incontestable proof of the existence of such oscillations, although the differences which these oscillations produce at different hours of the day are far smaller than the casual changes, hitherto reduced to no law, which go on from hour to hour and from day to day. The effect of

\* *Phil. Trans.* 1831.

† This period of nineteen years was also selected for a reason which is alluded to in a former note. (p. 406.) It was thought that this period secured the inquirer from the errors which might be produced by the partial coincidence of the arguments of different irregularities; for example, those due to the moon's parallax and to the moon's declination. It has since been found (*Phil. Tr.* 1838. *On the Determination of the Laws of the Tides from Short Series of Observations*.) that with regard to parallax at least, the Means of one year give sufficient accuracy.



law, operating incessantly and steadily, makes itself more and more felt as we give it a longer range; while the effect of accident, followed out in the same manner, is to annihilate itself, and to disappear altogether from the result.

### SECT. III.—*The Method of Least Squares.*

13. THE Method of Least Squares is in fact a method of means, but with some peculiar characters. Its object is to determine the *best Mean* of a number of observed quantities; or the *most probable Law* derived from a number of observations, of which some, or all, are allowed to be more or less imperfect. And the method proceeds upon this supposition;—that all errors are not *equally* probable, but that small errors are more probable than large ones. By reasoning mathematically upon this ground, we find that the best result is obtained (since we cannot obtain a result in which the errors vanish) by making, not the *Errors* themselves, but the *Sum of their Squares* of the *smallest* possible amount.

14. An example may illustrate this. Let a quantity which is known to increase uniformly, (as the distance of a star from the meridian at successive instants,) be measured at equal intervals of time, and be found to be successively 4, 12, 14. It is plain, upon the face of these observations that they are erroneous; for they ought to form an arithmetical progression, but they deviate widely from such a progression. But the question then occurs, what arithmetical progression do they *most probably* represent: for we may assume several arithmetical progressions which more or less approach the observed series; as for instance, these three; 4, 9, 14; 6, 10, 14; 5, 10, 15. Now in order to see the claims of each of these to the truth, we may tabulate them thus.

## Observation

|            | 4, 12, 14     | Errours.    | Sums of<br>Errours. | Sums of Squares<br>of Errours. |
|------------|---------------|-------------|---------------------|--------------------------------|
| Series (1) | 4, 9, 14 ...  | 0, 3, 0 ... | 3 ...               | 9                              |
| „ (2)      | 6, 10, 14 ... | 2, 2, 0 ... | 4 ...               | 8                              |
| „ (3)      | 5, 10, 15 ... | 1, 2, 1 ... | 4 ...               | 6                              |

Here, although the first series gives the sum of the errours less than the others, the third series gives the sum of the squares of the errours least; and is therefore, by the proposition on which this Method depends, the *most probable* series of the three.

This Method, in more extensive and complex cases, is a great aid to the calculator in his inferences from facts, and removes much that is arbitrary in the Method of Means.

SECT. IV.—*The Method of Residues.*

15. BY either of the preceding Methods we obtain, from observed facts, such laws as readily offer themselves; and by the laws thus discovered, the most prominent changes of the observed quantities are accounted for. But in many cases we have, as we have noticed already, *several* laws of nature operating at the same time, and combining their influences to modify those quantities which are the subjects of observation. In these cases we may, by successive applications of the Methods already pointed out, detect such laws one after another: but this successive process, though only a repetition of what we have already described, offers some peculiar features which make it convenient to consider it in a separate Section, as the Method of Residues.

16. When we have, in a series of changes of a variable quantity, discovered *one* Law which the changes follow, detected its argument, and determined its magnitude so as to explain most clearly the course of observed facts, we may still find that the observed changes are not fully

accounted for. When we compare the results of our Law with the observations, there may be a difference, or as we may term it, a *Residue*, still unexplained. But this Residue being thus detached from the rest, may be examined and scrutinized in the same manner as the whole observed quantity was treated at first: and we may in this way detect in *it* also a Law of change. If we can do this, we must accommodate this new found Law as nearly as possible to the Residue to which it belongs; and this being done, the difference of our Rule and of the Residue itself, forms a *Second Residue*. This Second Residue we may again bring under our consideration; and may perhaps in *it* also discover some Law of change by which its alterations may be in some measure accounted for. If this can be done, so as to account for a large portion of this Residue, the remaining unexplained part forms a *Third Residue*; and so on.

17. This course has really been followed in various inquiries, especially in those of Astronomy and Tidology. The *Equation of the Center*, for the moon, was obtained out of the *Residue* of the Longitude, which remained when the *Mean Anomaly* was taken away. This Equation being applied and disposed of, the *Second Residue* thus obtained, gave to Ptolemy the *Evection*. The *Third Residue*, left by the Equation of the Center and the Evecton, supplied to Tycho the *Variation* and the *Annual Equation*. And the Residue, remaining from these, has been exhausted by other equations, of various arguments, suggested by theory or by observation. In this case, the successive generations of astronomers have gone on, each in its turn executing some step in this Method of Residues. In the examination of the Tides, on the other hand, this method has been applied systematically and at once. The observations readily gave the *Semimensual Inequality*; the *Residue* of this

supplied the corrections due to the Moon's *Parallax* and *Declination*; and when these were determined, the *remaining Residue* was explored for the law of the Solar Correction.

18. In a certain degree, the Method of Residues and the Method of Means are *opposite* to each other. For the Method of Residues extricates Laws from their combination, *bringing them into view in succession*; while the Method of Means discovers each Law, not by bringing the others into view, but by *destroying their effect* through an accumulation of observations. By the Method of Residues we should *first* extract the Law of the Parallax Correction of the Tides, and *then*, from the Residue left by this, obtain the Declination Correction. But we might at once employ the Method of Means, and put together all the cases in which the Declination was the same; not allowing for the Parallax in each case, but taking for granted that the Parallaxes belonging to the same Declination would neutralize each other; as many falling above as below the mean parallax. In cases like this, where the Method of Means is not impeded by a partial coincidence of the Arguments of different unknown Inequalities, it may be employed with almost as much success as the Method of Residues. But still, when the Arguments of the Laws are clearly known, as in this instance, the Method of Residues is more clear and direct, and is the rather to be recommended.

19. If for example, we wish to learn whether the Height of the Barometer exerts any sensible influence on the Height of the Sea's Surface, it would appear that the most satisfactory mode of proceeding, must be to subtract, in the first place, what we know to be the effects of the Moon's Age, Parallax and Declination, and other ascertained causes of change; and to search in the *unexplained Residue* for the effects of barometrical pressure. The con-

trary course has, however, been adopted, and the effect of the barometer on the ocean has been investigated by the direct application of the Method of Means, classing the observed heights of the water according to the corresponding heights of the barometer without any previous reduction. In this manner, the suspicion that the tide of the sea is effected by the pressure of the atmosphere, has been confirmed. This investigation must be looked upon as a remarkable instance of the efficacy of the Method of Means, since the amount of the barometrical effect is much smaller than the other changes from among which it was by this process extricated. But an application of the Method of Residues would still be desirable on a subject of such extent and difficulty.

20. Sir John Herschel, in his *Discourse on the Study of Natural Philosophy* (Articles 158—161), has pointed out the mode of making discoveries by studying Residual Phenomena; and has given several illustrations of the process. In some of these, he has also considered this method in a wider sense than we have done; treating it as not applicable to quantity only, but to properties and relations of different kinds.

We likewise shall proceed to offer a few remarks on Methods of Induction applicable to other relations than those of quantity.

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## CHAPTER VIII.

### METHODS OF INDUCTION DEPENDING ON RESEMBLANCE.

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#### SECT. I.—*The Law of Continuity.*

1. THE Law of Continuity is applicable to quantity primarily, and therefore might be associated with the

methods treated of in the last chapter: but inasmuch as its inferences are made by a transition from one degree to another among contiguous cases, it will be found to belong more properly to the Methods of Induction of which we have now to speak.

The *Law of Continuity* consists in this proposition,—That a quantity cannot pass from one amount to another by any change of conditions, without passing through all intermediate degrees of magnitude according to the intermediate conditions. And this law may often be employed to correct inaccurate inductions, and to reject distinctions which have no real foundation in nature. For example, the Aristotelians made a distinction between motions according to nature, as that of a body falling vertically downwards, and motions contrary to nature, as that of a body moving along a horizontal plane: the former, they held, became naturally quicker and quicker, the latter naturally slower and slower. But to this it might be replied, that a horizontal line may pass, by gradual motion, through various inclined positions, to a vertical position: and thus the retarded motion may pass into the accelerated; and hence there must be some inclined plane on which the motion downwards is naturally uniform: which is false, and therefore the distinction of such kinds of motion is unfounded. Again, the proof of the First Law of Motion depends upon the Law of Continuity: for since, by diminishing the resistance to a body moving on a horizontal plane, we diminish the retardation, and this without limit, the law of continuity will bring us at the same time to the case of no resistance and to the case of no retardation.

2. The Law of Continuity is asserted by Galileo in a particular application; and the assertion which it suggests is by him referred to Plato;—namely\*, that a moveable

\* *Dialog.* III. 150. IV. 32.

body cannot pass from rest to a determinate degree of velocity without passing through all smaller degrees of velocity. This law, however, was first asserted in a more general and abstract form by Leibnitz\*: and was employed by him to show that the laws of motion propounded by Descartes must be false. The Third Cartesian Law of Motion was this†: that when one moving body meets another, if the first body have a less momentum than the second, it will be reflected with its whole motion: but if the first have a greater momentum than the second, it will lose a part of its motion, which it will transfer to the second. Now each of these cases leads, by the Law of Continuity, to the case in which the two bodies have *equal* momentums: but in this case, by the first part of the law the body would *retain all* its motion; and by the second part of the law it would *lose* a portion of it: hence the Cartesian Law is false.

3. I shall take another example of the application of this Law from Professor Playfair's Dissertation on the History of Mathematical and Physical Science‡. "The Academy of Sciences at Paris having (in 1724) proposed, as a Prize Question, the Investigation of the Laws of the Communication of Motion, John Bernoulli presented an Essay on the subject very ingenious and profound; in which, however, he denied the existence of hard bodies, because in the collision of such bodies, a finite change of motion must take place in an instant: an event which, on the principle just explained, he maintained to be impossible." And this reasoning was justifiable: for we can form a *continuous* transition from cases in which the impact manifestly occupies a finite time, (as when we strike a large soft body) to cases in which it is apparently instantaneous. Maclaurin and others are disposed, in

\* *Opera*, i. 366.

† *Cartes. Prin.*, p. 35.

‡ In the *Encyc. Brit.*, p 537.

order to avoid the conclusion of Bernoulli, to reject the Law of Continuity. This, however, would not only be, as Playfair says, to deprive ourselves of an auxiliary, commonly useful though sometimes deceptive; but what is much worse, to acquiesce in false propositions, from the want of clear and patient thinking. For the Law of Continuity, when rightly interpreted, is *never* violated in actual fact. There are not really any such bodies as have been termed *perfectly hard*: and if we approach towards such cases, we must learn the laws of motion which rule them by attending to the Law of Continuity, not by rejecting it.

4. Newton used the Law of Continuity to suggest, but not to prove, the doctrine of universal gravitation. Let, he said, a terrestrial body be carried as high as the moon: will it not still fall to the earth? and does not the moon fall by the same force\*? Again: if any one says that there is a material ether which does not gravitate†, this kind of matter, by condensation, may be gradually transmuted to the density of the most intensely gravitating bodies: and these gravitating bodies, by taking the internal texture of the condensed ether, may cease to gravitate; and thus the weight of bodies depends, not on their quantity of matter, but on their texture; which doctrine Newton conceived he had disproved by experiment.

5. The evidence of the Law of Continuity resides in the universality of those ideas, which enter into our apprehension of Laws of Nature. When, of two quantities, one depends upon the other, the Law of Continuity necessarily governs this dependence. Every philosopher has the power of applying this law, in proportion as he has the faculty of apprehending the ideas which he employs in his induction, with the same clearness and steadiness

\* *Principia*, Lib. III. Prop. 6.

† *Ib.*, Cor. 2.



which belong to the fundamental ideas of quantity, space and number. To those who possess this faculty, the Law is a Rule of very wide and decisive application. Its use, as has appeared in the above examples, is seen rather in the disproof of erroneous views, and in the correction of false propositions, than in the invention of new truths. It is a test of truth, rather than an instrument of discovery.

Methods, however, approaching very near to the Law of Continuity may be employed as positive means of obtaining new truths; and these I shall now describe.

#### SECT. II.—*The Method of Gradation.*

6. To gather together the cases which resemble each other, and to separate those which are essentially distinct, has often been described as the main business of science; and may, in a certain loose and vague manner of speaking, pass for a description of some of the leading procedures in the acquirement of knowledge. The selection of instances which agree, and of instances which differ, in some prominent point or property, are important steps in the formation of science. But when classes of things and properties have been established in virtue of such comparisons, it may still be doubtful whether these classes are separated by distinctions of opposites, or by differences of degree. And to settle such questions, the *Method of Gradation* is employed; which consists in taking intermediate stages of the properties in question, so as to ascertain by experiment whether, in the transition from one class to another, we have to leap over a manifest gap, or to follow a continuous road.

7. Thus for instance, one of the early *Divisions* established by electrical philosophers was that of *Electrics* and *Conductors*. But this division Faraday has overturned as an essential opposition. He takes \* a *Gradation* which

\* *Researches*, 12th Series, Art. 132B.

carries him from Conductors to Non-conductors. Sulphur, or lac, he says, are held to be non-conductors, but are not rigorously so. Spermaceti is a bad conductor: ice or water better than spermaceti: metals so much better that they are put in a different class. But even in metals the transit of the electricity is not instantaneous: we have in them proof of a retardation of the electric current: "and what reason," Mr. Faraday asks, "why this retardation should not be of the same kind as that in spermaceti, or in lac, or sulphur? But as, in them, retardation is insulation, [and insulation is induction\*] why should we refuse the same relation to the same exhibitions of force in the metals?"

The process employed by the same sagacious philosopher to show the *identity* of Voltaic and Franklinic electricity, is another example of the same kind†. Machine [Franklinic] electricity was made to exhibit the same phenomena as Voltaic electricity, by causing the discharge to pass through a bad conductor, into a very extensive discharging train: and thus it was clearly shown that Franklinic electricity, not so conducted, differs from the other kinds, only in being in a state of successive tension and explosion instead of a state of continued current.

Again; to show that the decomposition of bodies in the Voltaic circuit was not due to the Attraction of the Poles‡, Mr. Faraday devised a beautiful series of experiments, in which these supposed *Poles* were made to assume all possible electrical conditions:—in which the decomposition took place against air, which according to common language is not a conductor, nor is decomposed;—against the metallic poles, which are excellent conduc-

\* These words refer to another proposition, also established by the Method of Gradation. † *Hist. Ind. Sci.*, B. xiv. c. ix. sect. 2.

‡ *Ibid.*, *Researches*, Art. 497.

tors but undecomposable: and hence he infers that the decomposition cannot justly be considered as due to the Attraction, or Attractive Powers, of the Poles.

8. The reader of the *Novum Organon* may perhaps, in looking at such examples of the Rule, be reminded of some of Bacon's classes of instances, as his *instantiæ absentiæ in proximo*, and his *instantiæ migrantes*. But we may remark that instances classed and treated as Bacon recommends in those parts of his work, could hardly lead to scientific truth. His processes are vitiated by his proposing to himself the *form* or *cause* of the property before him, as the object of his enquiry; instead of being content to obtain, in the first place, the *law of phenomena*. Thus his example\* of a migrating instance is thus given. "Let the *nature inquired into* be that of whiteness; an instance migrating to the production of this property is glass, first whole, and then pulverized; or plain water, and water agitated into a foam; for glass and water are transparent, and not white; but glass powder and foam are white, and not transparent. Hence we must inquire what has happened to the glass or water in that migration. For it is plain that the *form of whiteness* is conveyed and induced by the crushing of the glass and shaking of the water."

9. We may easily give examples from other subjects in which the method of gradation has been used to establish, or to endeavour to establish, very extensive propositions. Thus Laplace's Nebular Hypothesis,—that systems like our solar system are formed by gradual condensation from diffused masses, such as the nebulæ among the stars,—is founded by him upon an application of this Method of Gradation. We see, he conceives, among these nebulæ, instances of all degrees of condensation, from the most loosely diffused fluid, to that separation

\* *Nov. Org.*, Lib. II. Aph. 28.

and solidification of parts by which suns, and satellites, and planets are formed: and thus we have before us instances of systems in all their stages; as in a forest we see trees in every period of growth. How far the examples in this case satisfy the demands of the Method of Gradation, it remains for astronomers and philosophers to examine.

Again; this method was used with great success by Macculloch and others to refute the opinion, put in currency by the Wernerian school of geologists, that the rocks called *trap rocks* must be classed with those to which a *sedimentary* origin is ascribed. For it was shown that a gradual *transition* might be traced from those examples in which trap rocks most resembled stratified rocks, to the lavas which have been recently ejected from volcanoes: and that it was impossible to assign a different origin to one portion, and to the other, of this kind of mineral masses; and as the volcanic rocks were certainly not *sedimentary*, it followed, that the trap rocks were not of that nature.

Again; we have an attempt of a still larger kind made by Mr. Lyell, to apply this Method of Gradation so as to disprove all distinction between the causes by which geological phenomena have been produced, and the causes which are now acting at the earth's surface. He has collected a very remarkable series of changes which have taken place, and are still taking place, by the action of water, volcanoes, earthquakes, and other terrestrial operations; and he conceives he has shown in these a *gradation* which leads, with no wide chasm or violent leap, to the state of things of which geological researches have supplied the evidence.

10. Of the value of this Method in geological speculations, no doubt can be entertained. Yet it must still require a grave and profound consideration, in so vast an

application of the Method as that attempted by Mr. Lyell, to determine what extent we may allow to the steps of our *gradation* ; and to decide how far the changes which have taken place in distant parts of the series may exceed those of which we have historical knowledge, before they cease to be of the *same kind*. Those who, dwelling in a city, see, from time to time, one house built and another pulled down, may say that such *existing causes*, operating through past time, sufficiently explain the existing condition of the city. Yet we arrive at important political and historical truths, by considering the *origin* of a city as an event of a *different order* from those daily changes. The causes which are now working to produce geological results, may be supposed to have been, at some former epoch, so far exaggerated in their operation, that the changes should be paroxysms, not degrees;—that they should violate, not continue, the gradual series. And we have no kind of evidence whether the duration of our historical times is sufficient to give us a just measure of the limits of such degrees;—whether the terms which we have under our notice enable us to ascertain the average rate of progression.

11. The result of such considerations seems to be this:—that we may apply the Method of Gradation in the investigation of geological causes, provided we leave the Limits of the Gradation undefined. But, then, this is equivalent to the admission of the opposite hypothesis: for a continuity of which the successive intervals are not limited, is not distinguishable from discontinuity. The geological sects of recent times have been distinguished as *uniformitarians* and *catastrophists*: the Method of Gradation seems to prove the doctrine of the uniformitarians; but then, at the same time that it does this, it breaks down the distinction between them and the catastrophists.

There are other exemplifications of the use of gradations in Science which well deserve notice: but some of them are of a kind somewhat different, and may be considered under a separate head.

### SECT. III. *The Method of Natural Classification.*

12. THE method of natural classification consists, as we have seen, in grouping together objects, not according to any selected properties, but according to their most important resemblances; and in combining such grouping with the assignation of certain marks of the classes thus formed. The examples of the successful application of this method are to be found in the Classificatory Sciences through their whole extent; as, for example, in framing the Genera of plants and animals. The same method, however, may often be extended to other sciences. Thus the classification of crystalline forms, according to their degree of symmetry, (which is really an important distinction,) as introduced by Mohs and Weiss, was a great improvement upon Haüy's arbitrary division according to certain assumed primary forms. Sir David Brewster was led to the same distinction of crystals by the study of their optical properties; and the scientific value of the classification was thus strongly exhibited. Mr. Howard's classification of clouds appears to be founded in their real nature, since it enables him to express the laws of their changes and successions. As we have elsewhere said, the criterion of a true classification is, that it makes general propositions possible. One of the most prominent examples of the beneficial influence of a right classification, is to be seen in the impulse given to geology by the distinction of strata according to the organic fossils which they contain\*:

\* *Hist. Ind. Sci.*, B. XVIII. c. ii. sect. 3.

which, ever since its general adoption, has been a leading principle in the speculations of geologists.

13. The mode in which, in this and in other cases, the Method of Natural Classification directs the researches of the philosopher, is this:—his arrangement being adopted, at least as an instrument of inquiry and trial, he follows the course of the different members of the classification, according to the guidance which Nature herself offers; not prescribing beforehand the marks of each part, but distributing the facts according to the total resemblances, or according to those resemblances which he finds to be most important. Thus, in tracing the course of a series of strata from place to place, we identify each stratum, not by any single character, but by all taken together;—texture, colour, fossils, position, and any other circumstances which offer themselves. And if, by this means, we come to ambiguous cases, where different indications appear to point different ways, we decide so as best to preserve undamaged those general relations and truths which constitute the value of our system. Thus although we consider the organic fossils in each stratum as its most important characteristic, we are not prevented, by the disappearance of some fossils, or the addition of others, or by the total absence of fossils, from identifying strata in distant countries, if the position and other circumstances authorize us to do so. And by this Method of Classification, the doctrine of *Geological Equivalents*\* has been applied to a great part of Europe.

14. We may further observe, that the same method of natural classification which thus enables us to identify strata in remote situations, notwithstanding there may be great differences in their material and contents, also forbids us to assume the identity of the series of rocks which

\* *Hist. Ind. Sci.*, B. XVIII. c. iii. sect. 4.

occur in different countries, when this identity has not been verified by such a continuous exploration of the component members of the series. It would be in the highest degree unphilosophical to apply the special names of the English or German strata to the rocks of India, or America, or even of southern Europe, till it has appeared that in those countries the geological series of northern Europe really exists. In each separate country, the divisions of the formations which compose the crust of the earth must be made out, by applying the Method of Natural Arrangement *to that particular case*, and not by arbitrarily extending to it the nomenclature belonging to another case. It is only by such precautions, that we can ever succeed in obtaining geological propositions, at the same time true and comprehensive; or can obtain any sound general views respecting the physical history of the earth.

15. The Method of Natural Classification, which we thus recommend, falls in with those mental habits which we formerly described as resulting from the study of natural history. The method was then termed the *Method of Type*, and was put in opposition to the *Method of Definition*.

The Method of Natural Classification is directly opposed to the process in which we assume and apply *arbitrary* definitions; for in the former Method, we find our classes in nature, and do not make them by marks of our own imposition. Nor can any advantage to the progress of knowledge be procured, by laying down our characters when our arrangements are as yet quite loose and unformed. Nothing was gained by the attempts to *define* Metals by their weight, their hardness, their ductility, their colour; for to all these marks, as fast as they were proposed, exceptions were found, among bodies which still could not be excluded from the list of Metals. It



was only when elementary substances were divided into *Natural Classes*, of which classes Metals were one, that a true view of their distinctive characters was obtained. Definitions in the outset of our examination of nature are almost always, not only useless, but prejudicial.

16. When we obtain a law of nature by induction from phenomena, it commonly happens, as we have already seen, that we introduce, at the same time, a Proposition and a Definition. In this case, the two are correlative, each giving a real value to the other. In such cases, also, the Definition, as well as the Proposition, may become the basis of rigorous reasoning, and may lead to a series of deductive truths. We have examples of such Definitions and Propositions in the laws of motion, and in many other cases.

17. When we have established Natural Classes of objects, we seek for Characters of our classes; and these Characters may, to a certain extent, be called the *Definitions* of our classes. This is to be understood, however, only in a limited sense: for these Definitions are not absolute and permanent. They are liable to be modified and superseded. If we find a case which manifestly belongs to our Natural Class, though violating our Definition, we do not shut out the case, but alter our definition. Thus, when we have made it part of our Definition of the *Rose* family, that they have *alternate stipulate leaves*, we do not, therefore, exclude from the family the genus *Lonicæa*, which has *no stipulæ*. In Natural Classifications, our Definitions are to be considered as temporary and provisional only. When Mr. Lyell established the distinctions of the tertiary strata, which he termed *Eocene*, *Miocene*, and *Pliocene*, he took a numerical criterion (the proportion of recent species of shells contained in those strata) as the basis of his division. But now that those kinds of strata have become, by their

application to a great variety of cases, a series of Natural Classes, we must, in our researches, keep in view the natural connexion of the formations themselves in different places; and must by no means allow ourselves to be governed by the numerical proportions which were originally contemplated; or even by any amended numerical criterion equally arbitrary; for however amended, Definitions in natural history are never immortal. The etymologies of *Pliocene* and *Miocene* may, hereafter, come to have merely an historical interest; and such a state of things will be no more inconvenient, provided the natural connexions of each class are retained, than it is to call a rock *oolite* or *porphyry*, when it has no roelike structure and no fiery spots.

The Methods of Induction which are treated of in this and the preceding chapter, and which are specially applicable to causes governed by relations of Quantity or of Resemblance, commonly lead us to *Laws of Phenomena* only. Inductions founded upon other ideas, those of Substance and Cause for example, appear to conduct us somewhat further into a knowledge of the essential nature and real connexions of things. But before we speak of these, we shall say a few words respecting the way in which inductive propositions, once obtained, may be verified and carried into effect by their application.

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## CHAPTER IX.

### OF THE APPLICATION OF INDUCTIVE TRUTHS.

1. BY the application of inductive truths, we here mean, according to the arrangement given in Chap. I. of this Book, those steps, which in the natural order of science, follow the discovery of each truth. These steps

are, the *verification* of the discovery by additional experiments and reasonings, and its *extension* to new cases, not contemplated by the original discoverer. These processes occupy that period, which, in the history of each great discovery, we have termed the *Sequel* of the epoch; as the collection of facts, and the elucidation of conceptions, form its Prelude.

2. It is not necessary to dwell at length on the processes of the verification of discoveries. When the law of nature is once stated, it is far easier to devise and execute experiments which prove it, than it was to discern the evidence before. The truth becomes one of the standard doctrines of the science to which it belongs, and is verified by all who study or who teach the science experimentally. The leading doctrines of chemistry are constantly exemplified by each chemist in his *Laboratory*; and an amount of verification is thus obtained of which books give no adequate conception. In astronomy, we have a still stronger example of the process of verifying discoveries. Ever since the science assumed a systematic form, there have been *Observatories*, in which the consequences of the theory were habitually compared with the results of observation. And to facilitate this comparison, *Tables* of great extent have been calculated, with immense labour, from each theory, showing the place which the theory assigned to the heavenly bodies at successive times; and thus, as it were, challenging nature to deny the truth of the discovery. In this way, as I have elsewhere stated, the continued prevalence of an error in the systematic parts of astronomy is impossible\*. An error, if it arise, makes its way into the tables, into the ephemeris, into the observer's nightly list, or his sheet of reductions; the evidence of sense flies in its face in a thousand Observatories; the dis-

\* *Hist. Ind. Sci.*, B. VII. c. vi. sect. 6.

crepancy is traced to its source, and soon disappears for ever.

3. In these last expressions, we suppose the theory, not only to be tested, but also to be corrected when it is found to be imperfect. And this also is part of the business of the observing astronomer. From his accumulated observations, he deduces more exact values than had previously been obtained, of the *Coefficients* of these Inequalities of which the *Argument* is already known. This he is enabled to do by the methods explained in the fifth chapter of this Book; the Method of Means, and especially the Method of Least Squares. In other cases, he finds, by the Method of Residues, some new Inequality; for if no change of the Coefficients will bring the Tables and the observation to a coincidence, he knows that a new Term is wanting in his formula. He obtains, as far as he can, the law of this unknown Term; and when its existence and its law have been fully established, there remains the task of tracing it to its cause.

4. The condition of the science of Astronomy, with regard to its security and prospect of progress, is one of singular felicity. It is a question well worth our consideration, as regarding the interests of science, whether, in other branches of knowledge also, *a continued and connected system of observation and calculation*, imitating the system employed by astronomers, might not be adopted. But the discussion of this question would involve us in a digression too wide for the present occasion.

5. There is another mode of application of true theories after their discovery, of which we must also speak; I mean the process of showing that facts, not included in the original induction, and apparently of a different kind, are explained by reasonings founded upon the theory. The history of physical astronomy is full of such events.

Thus after Bradley and Wargentin had observed a certain cycle among the perturbations of Jupiter's satellites, Laplace explained this cycle by the doctrine of universal gravitation\*. The long inequality of Jupiter and Saturn, the diminution of the obliquity of the ecliptic, the acceleration of the moon's mean motion, were in like manner accounted for by Laplace. The coincidence of the nodes of the moon's equator with those of her orbit was proved to result from mechanical principles by Lagrange. The motions of the recently-discovered planets, and of comets, shown by various mathematicians to be in exact accordance with the theory, are verifications and extensions still more obvious.

6. In many of the cases just noticed, the consistency between the theory, and the consequences thus proved to result from it, is so far from being evident, that the most consummate command of all the powers and aids of mathematical reasoning is needed, to enable the philosopher to arrive at the result. In consequence of this circumstance, the labours just referred to, of Laplace, Lagrange, and others, have been the object of very great and very just admiration. Moreover, the necessary connexion of new facts, at first deemed inexplicable, with principles already known to be true;—a connexion utterly invisible at the outset, and yet at last established with the certainty of demonstration;—strikes us with the delight of a new discovery; and at first sight appears no less admirable than an original induction. Accordingly, men sometimes appear tempted to consider Laplace and other great mathematicians as persons of a kindred genius to Newton. We must not forget, however, that there is a great and essential difference between inductive and deductive processes of the mind. The discovery of a *new* theory, which is true, is a step widely distinct

\* *Hist. Ind. Sci.*, B. vii. c. iv. sect. 3.

from any mere development of the consequences of a theory already invented and established.

7. As an example, in another field, of the extension of a discovery by applying it to the explanation of new phenomena, we may adduce Wells's *Inquiry into the Cause of Dew*. For this investigation, although it has sometimes been praised as an original discovery, was, in fact, only resolving the phenomenon into principles already discovered. The atmologists of the last century were aware\* that the vapour which exists in air in an invisible state may be condensed into water by cold; and they had noticed that there is always a certain temperature, lower than that of the atmosphere, to which if we depress bodies, water forms upon them in fine drops. This temperature is the limit of that which is necessary to constitute vapour, and is hence called the *constituent temperature*. But these principles were not generally familiar in England till Dr. Wells introduced them into his *Essay on Dew*, published in 1814; having indeed been in a great measure led to them by his own experiments and reasonings. His explanation of Dew,—that it arises from the coldness of the bodies on which it settles,—was established with great ingenuity; and is a very elegant confirmation of the Theory of Constituent Temperature.

8. The example of all the best writers who have previously treated of the philosophy of sciences, from Bacon to Herschel, draws our attention to those instances of the application of scientific truths, which are subservient to the uses of practical life; to the support, the preservation, the pleasure of man. It is well known in how large a degree the furtherance of these objects constituted the merit of the *Novum Organon* in the eyes of its author; and the enthusiasm with which men regard

\* *Hist. Ind. Sci.*, B. x. c. iii. sect. 5.

these visible and tangible manifestations of the power and advantage which knowledge may bring, has gone on increasing up to our own day. Such useful inventions as we here refer to must always be objects of great philosophical, as well as practical interest; and it might be well worth our while, did our present limits allow, to discuss the bearing of such inventions upon the formation and progress of science. For the present, it must suffice to observe that those practical inventions which are of most importance in the Arts, are rarely or never of any material consequence to Science; for they are either mere practical processes, which the artist practises, but which the scientist cannot account for: or at most, they depend upon some of the inferior generalizations of science for their reason, and do not tend to confirm or illustrate the higher points at which theory has arrived. These considerations must be our apology for not entering into this discussion at the present advanced stage of our undertaking. As we have already said, knowledge is power; but its interest for us in the present work, is not that it is power, but that it is knowledge. The effect which the application of science to general practical uses has in *diffusing* a knowledge of theoretical principles, and thus in giving to men's minds an intellectual culture, is indeed well worthy our attention; but the consideration of this subject must be reserved for some future occasion.

We must now conclude our task by a few words on the subject of inductions involving Ideas ulterior to those already considered.

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## CHAPTER X.

## OF THE INDUCTION OF CAUSES.

1. WE formerly\* stated the objects of the researches of Science to be Laws of Phenomena and Causes; and showed the propriety and the necessity of not resting in the former object, but extending our inquiries to the latter also. Inductions, in which phenomena are connected by relations of Space, Time, Number and Resemblance, belong to the former class; and of the Methods applicable to such Inductions we have treated already. In proceeding to Inductions governed by any ulterior Ideas, we can no longer lay down any Special Methods by which our procedure may be directed. A few general remarks are all that we shall offer.

The principal Maxim in such cases of Induction is the obvious one:—that we must be careful to possess and to apply, with perfect clearness and precision, the Fundamental Idea on which the Induction depends.

We may illustrate this in a few cases.

2. *Induction of Substance.*—The Idea of Substance† involves this axiom, that the weight of the whole compound must be equal to the weights of the separate elements, whatever changes the composition or separation of the elements may have occasioned. The application of this Maxim we may term the *Method of the Balance*. We have seen‡ how the memorable revolution in Chemistry, the overthrow of phlogiston, and the establishment of the oxygen theory, was produced by the application of this Method. We have seen too§ that the same Idea leads us to this Maxim;—that *Imponderable Fluids* are not to be admitted as *chemical* elements of bodies.

\* Book XI. c. vii.

† *Ibid.*, B. VI. c. iv.

‡ *Ibid.*, VI. c. iii.

§ *Ibid.*



Whether those which have been termed *Imponderable Fluids*,—the supposed fluids which produce the phenomena of Light, Heat, Electricity, Galvanism, Magnetism,—really exist or no, is a question, not merely of the *Laws*, but of the *Causes* of Phenomena. It is, as has already been shown, a question which we cannot help discussing, but which is at present involved in great obscurity. Nor does it appear at all likely that we shall obtain a true view of the cause of Light, Heat, and Electricity, till we have discovered precise and general laws connecting optical, thermotical, and electrical *phenomena* with those chemical doctrines to which the Idea of Substance is necessarily applied.

3. *Induction of Force*.—The inference of *Mechanical Forces* from phenomena has been so abundantly practised, that it is perfectly familiar among scientific inquirers. From the time of Newton, it has been the most common aim of mathematicians; and a persuasion has grown up among them, that mechanical forces,—attraction and repulsion,—are the only modes of action of the particles of bodies which we shall ultimately have to consider. I have attempted to show that this mode of conception is inadequate to the purposes of sound philosophy;—that the particles of crystals, and the elements of chemical compounds, must be supposed to be combined in some other way than by mere mechanical attraction and repulsion. Dr. Faraday has gone further in shaking the usual conceptions of the force exerted, in well-known cases. Among the most noted and conspicuous instances of attraction and repulsion exerted at a distance, were those which take place between electrized bodies. But the eminent electrician just mentioned has endeavoured to establish, by experiments of which it is very difficult to elude the weight, that the action in these cases does not take place at a distance, but is the result of a chain

of intermediate particles connected at every point by forces of another kind.

4. *Induction of Polarity*.—The forces to which Mr. Faraday ascribes the action in these cases are *Polar Forces*. We have already endeavoured to explain the Idea of Polar Forces; which implies† that at every point forces exactly equal act in opposite directions; and thus, in the greater part of their course, neutralize and conceal each other; while at the extremities of the line, being by some cause liberated, they are manifested, still equal and opposite. And the criterion by which this polar character of forces is recognized, is implied in the reasoning of Faraday, on the question of one or two electricities, of which we formerly spoke‡. The maxim is this:—that in the action of polar forces, along with every manifestation of force or property, there exists a corresponding and simultaneous manifestation of an equal and opposite force or property.

5. As it was the habit of the last age to reduce all action to mechanical forces, the present race of physical speculators appears inclined to reduce all forces to polar forces. Mosotti has endeavoured to show that the positive and negative electricities pervade all bodies, and that gravity is only an apparent excess of one of the kinds over the other. As we have seen, Faraday has given strong experimental grounds for believing that the supposed remote actions of electrized bodies are really the effects of polar forces among contiguous particles. If this doctrine were established with regard to all electrical, magnetical, and chemical forces, we might ask, whether, while all other forces are polar, gravity really affords a single exception to the universal rule? Is not the universe pervaded by an omnipresent anta-

\* *Researches*, 12th Series.

† B. v. c. i.

‡ *Ibid.*

gonism, a fundamental conjunction of contraries, everywhere opposite, nowhere independent? We are, as yet, far from the position in which Inductive Science can enable us to answer such inquiries.

6. *Induction of Utterior Causes.*—The first Induction of a Cause does not close the business of scientific inquiry. Behind proximate causes, there are ulterior causes, perhaps a succession of such. Gravity is the cause of the motions of the planets; but what is the cause of gravity? This is a question which has occupied men's minds from the time of Newton to the present day. Earthquakes and volcanoes are the causes of many geological phenomena; but what is the cause of those subterraneous operations? This inquiry after ulterior causes is an inevitable result from the intellectual constitution of man. He discovers mechanical causes, but he cannot rest in them. He must needs ask, whence it is that matter has its universal power of attracting matter. He discovers polar forces: but even if these be universal, he still desires a further insight into the cause of this polarity. He sees, in organic structures, convincing marks of adaptation to an end: whence, he asks, is this adaptation? He traces in the history of the earth a chain of causes and effects operating through time: but what, he inquires, is the power which holds the end of this chain?

Thus we are referred back from step to step, in the order of causation, in the same manner as, in the palætiological sciences, we were referred back in the order of time. We make discovery after discovery in the various regions of science; each, it may be, satisfactory, and in itself complete, but none final. Something always remains undone. The last question answered, the answer suggests still another question. The strain of music from the lyre of Science flows on, rich and sweet, full

and harmonious, but never reaches a close: no cadence is heard with which the intellectual ear can feel satisfied.

*Of the Supreme Cause.*—In the utterance of Science, no cadence is heard with which the human mind can feel satisfied. Yet we cannot but go on listening for and expecting a satisfactory close. The notion of a cadence appears to be essential to our relish of the music. The idea of some closing strain seems to lurk among our own thoughts, waiting to be articulated in the notes which flow from the knowledge of external nature. The idea of something ultimate in our philosophical researches, something in which the mind can acquiesce, and which will leave us no further questions to ask, of *whence*, and *why*, and *by what power*, seems as if it belonged to us;—as if we could not have it withheld from us by any imperfection or incompleteness in the actual performances of science. What is the meaning of this conviction? What is the reality thus anticipated? Whither does the developement of this Idea conduct us?

We have already seen that a difficulty of the same kind, which arises in the contemplation of causes and effects considered as forming an historical series, drives us to the assumption of a First Cause, as an Axiom to which our Idea of Causation in time necessarily leads. And as we were thus guided to a First Cause in order of Succession, the same kind of necessity directs us to a Supreme Cause in order of Causation.

On this most weighty subject it is difficult to speak fitly; and the present is not the proper occasion, even for most of that which may be said. But there are one or two remarks which flow from the general train of the contemplations we have been engaged in, and with which this Work must conclude.

We have seen how different are the kinds of cause to which we are led by scientific researches. *Mechanical*

*Forces* are insufficient without *Chemical Affinities* ; Chemical agencies fail us, and we are compelled to have recourse to *Vital Powers* ; Vital Powers cannot be merely physical, and we must believe in something hyperphysical, something of the nature of a *Soul*. Not only do biological inquiries lead us to assume an animal soul, but they drive us much further ; they bring before us *Perception*, and *Will* evoked by Perception. Still more, these inquiries disclose to us *Ideas* as the necessary forms of Perception, in the actions of which we ourselves are conscious. We are aware, we cannot help being aware, of our Ideas and our Volitions as belonging to *us*, and thus we pass from *things* to *persons* ; we have the idea of *Personality* awakened. And the idea of Design and *Purpose*, of which we are conscious in our own minds, we find reflected back to us, with a distinctness which we cannot overlook, in all the arrangements which constitute the frame of organized beings.

We cannot but reflect how widely diverse are the kinds of principles thus set before us ;—by what vast strides we mount from the lower to the higher, as we proceed through that series of causes which the range of the sciences thus brings under our notice. Yet we know how narrow is the range of these sciences when compared with the whole extent of human knowledge. We cannot doubt that on many other subjects, besides those included in physical speculation, man has made out solid and satisfactory trains of connexion ;—has discovered clear and indisputable evidence of causation. It is manifest, therefore, that, if we are to attempt to ascend to the Supreme Cause—if we are to try to frame an idea of the Cause of all these subordinate causes ;—we must conceive it as more different from any of them, than the most diverse are from each other ;—more elevated above the highest, than the highest is above the lowest.

But further ;—though the Supreme Cause must thus be inconceivably different from all subordinate causes, and immeasurably elevated above them all, it must still include in itself all that is essential to each of them, by virtue of that very circumstance that it is the Cause of their Causality. Time and Space,—Infinite Time and Infinite Space,—must be among its attributes ; for we cannot but conceive Infinite Time and Space as attributes of the Infinite Cause of the Universe. Force and Matter must depend upon it for their efficacy ; for we cannot conceive the activity of Force, or the resistance of Matter, to be independent powers. But these are its lower attributes. The Vital Powers, the Animal Soul, which are the Causes of the actions of living things, are only the Effects of the Supreme Cause of Life. And this Cause, even in the lowest forms of organized bodies, and still more in those which stand higher in the scale, involves a reference to Ends and Purposes, in short, to manifest Final Causes. Since this is so, and since, even when we contemplate ourselves in a view studiously narrowed, we still find that we have Ideas, and Will and Personality, it would render our philosophy utterly incoherent and inconsistent with itself, to suppose that Personality, and Ideas, and Will, and Purpose, do not belong to the Supreme Cause from which we derive all that we have and all that we are.

But we may go a step further ;—though, in our present field of speculation, we confine ourselves to knowledge founded on the facts which the external world presents to us, we cannot forget, in speaking of such a theme as that to which we have thus been led, that these are but a small, and the least significant portion of the facts which bear upon it. We cannot fail to recollect that there are facts belonging to the world within us, which more readily and strongly direct our

thoughts to the Supreme Cause of all things. We can plainly discern that we have Ideas elevated above the region of mechanical causation, of animal existence, even of mere choice and will, which still have a clear and definite significance, a permanent and indestructible validity. We perceive as a fact, that we have a Conscience, judging of Right and Wrong; that we have Ideas of Moral Good and Evil; that we are compelled to conceive the organization of the moral world, as well as of the vital frame, to be directed to an end and governed by a purpose. And since the Supreme Cause is the cause of these facts, the Origin of these Ideas, we cannot refuse to recognize Him as not only the Maker, but the Governor of the World; as not only a Creative, but a Providential Power; as not only a Universal Father, but an Ultimate Judge.

We have already passed beyond the boundary of those speculations which we proposed to ourselves as the basis of our conclusions. Yet we may be allowed to add one other reflection. If we find in ourselves Ideas of Good and Evil, manifestly bestowed upon us to be the guides of our conduct, which guides we yet find it impossible consistently to obey;—if we find ourselves directed, even by our natural light, to aim at a perfection of our moral nature from which we are constantly deviating through weakness and perverseness;—if, when we thus lapse and err, we can find, in the region of human philosophy, no power which can efface our aberrations, or reconcile our actual with our ideal being, or give us any steady hope and trust with regard to our actions, after we have thus discovered their incongruity with their genuine standard;—if we discern that this is our condition, how can we fail to see that it is in the highest degree consistent with all the indications supplied by such a philosophy as that of which we have been

attempting to lay the foundations, that the Supreme Cause, through whom man exists as a moral being of vast capacities and infinite hopes, should have Himself provided a teaching for our ignorance, a propitiation for our sin, a support for our weakness, a purification and sanctification of our nature?

And thus, in concluding our long survey of the grounds and structure of science, and of the lessons which the study of it teaches us, we find ourselves brought to a point of view in which we can cordially sympathize, and more than sympathize, with all the loftiest expressions of admiration and reverence and hope and trust, which have been uttered by those who in former times have spoken of the elevated thoughts to which the contemplation of the nature and progress of human knowledge gives rise. We can not only hold with Galen, and Harvey, and all the great physiologists, that the organs of animals give evidence of a purpose;—not only assert with Cuvier that this conviction of a purpose can alone enable us to understand every part of every living thing;—not only say with Newton that “every true step made in philosophy brings us nearer to the First Cause, and is on that account highly to be valued;”—and that “the business of natural philosophy is to deduce causes from effects, till we come to the very First Cause, which certainly is not mechanical:”—but we can go much further, and declare, still with Newton, that “this beautiful system could have its origin no other way than by the purpose and command of an intelligent and powerful Being, who governs all things, not as the soul of the world, but as the Lord of the Universe; who is not only God, but Lord and Governor.”

When we have advanced so far, there yet remains one step. We may recollect the prayer of one, the master in this school of the philosophy of science: “This



also we humbly and earnestly beg;—that human things may not prejudice such as are divine;—neither that from the unlocking of the gates of sense, and the kindling of a greater natural light, anything may arise of incredulity or intellectual night towards divine mysteries; but rather that by our minds thoroughly purged and cleansed from fancy and vanity, and yet subject and perfectly given up to the divine oracles, there may be given unto faith the things that are faith's." When we are thus prepared for a higher teaching, we may be ready to listen to a greater than Bacon, when he says to those who have sought their God in the material universe, "Whom ye ignorantly worship, him declare I unto you." And when we recollect how utterly inadequate all human language has been shown to be, to express the nature of that Supreme Cause of the Natural, and Rational, and Moral, and Spiritual world, to which our Philosophy points with trembling finger and shaded eyes, we may receive, with the less wonder but with the more reverence, the declaration which has been vouchsafed to us:

ΕΝ ΑΡΧΗ ΗΝ 'Ο ΛΟΓΟΣ, ΚΑΙ 'Ο ΛΟΓΟΣ ΗΝ ΠΡΟΣ ΤΟΝ ΘΕΟΝ,  
ΚΑΙ ΘΕΟΣ ΗΝ 'Ο ΛΟΓΟΣ.

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# **APHORISMS.**

THE following Aphorisms exhibit some of the principal results of the views and discussions contained in the preceding pages of this work, expressed in a compact manner, and detached from the reasonings on which they rest. At the end of each Aphorism reference is made to the Book and Chapter where its import is discussed in the work.

Along with these, I shall add some other Aphorisms on the subject of the Language of Science; a subject in which it appears to be time to collect, from the usage of the most judicious writers, some rules which may tend to preserve the purity and analogies of scientific language from wanton and needless violation. As this subject is not discussed in the work itself, I have given, along with these Aphorisms, such examples as may tend to confirm and illustrate them, and have applied them to some cases at present unsettled.

## APHORISMS CONCERNING IDEAS.

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### I.

MAN is the Interpreter of Nature, Science the right interpretation. (Book I. Chapter I.)

### II.

The *Senses* place before us the *Characters* of the Book of Nature; but these convey no knowledge to us, till we have discovered the Alphabet by which they are to be read. (I. 2.)

### III.

The *Alphabet*, by means of which we interpret Phenomena, consists of the *Ideas* existing in our own minds; for these give to the phenomena that coherence and significance which is not an object of sense. (I. 2.)

### IV.

The antithesis of *Sense* and *Ideas* is the foundation of the Philosophy of Science. No knowledge can exist without the union, no philosophy without the separation, of these two elements. (I. 2.)

### V.

*Fact* and *Theory* correspond to Sense on the one hand, and to Ideas on the other, so far as we are *conscious* of our Ideas: but all facts involve ideas *unconsciously*; and thus the distinction of Facts and Theories is not tenable, as that of Sense and Ideas is. (I. 2.)

## VI.

Sensations and Ideas in our knowledge are like Matter and Form in bodies. Matter cannot exist without Form, nor Form without Matter: yet the two are altogether distinct and opposite. There is no possibility either of separating, or of confounding them. The same is the case with Sensations and Ideas. (I. 2.)

## VII.

Ideas are not *transformed*, but *informed* Sensations; for without ideas, sensations have no form. (I. 2.)

## VIII.

The Sensations are the *Objective*, the Ideas the *Subjective* part of every act of perception or knowledge. (I. 2.)

## IX.

General Terms denote *Ideal Conceptions*, as a *circle*, an *orbit*, a *rose*. These are not *Images* of real things, as was held by the Realists, but Conceptions: yet they are conceptions, not bound together by mere *Name*, as the Nominalists held, but by an Idea. (I. 2.)

## X.

It has been said by some, that all Conceptions are merely *states* or *feelings of the mind*, but this assertion only tends to confound what it is our business to distinguish. (I. 2.)

## XI.

Observed Facts are connected so as to produce new truths, by superinducing upon them an Idea: and such truths are obtained *by Induction*. (I. 2.)

## XII.

Truths once obtained by legitimate Induction are Facts: these Facts may be again connected, so as to produce higher truths: and thus we advance to *Successive Generalizations*. (I. 2.)

## XIII.

Truths obtained by Induction are made compact and permanent by being expressed in *Technical Terms*. (I. 3.)

## XIV.

Experience cannot conduct us to universal and necessary truths:—Not to universal, because she has not tried all cases:—Not to necessary, because necessity is not a matter to which experience can testify. (I. 5.)

## XV.

Necessary truths derive their necessity from the *Ideas* which they involve; and the existence of necessary truths proves the existence of Ideas not generated by experience. (I. 5.)

## XVI.

In Deductive Reasoning, we cannot have any truth in the conclusion which is not virtually contained in the premises. (I. 6.)

## XVII.

In order to acquire any exact and solid knowledge, the student must possess with perfect precision the ideas appropriate to that part of knowledge: and this precision is tested by the student's *perceiving* the axiomatic evidence of the *axioms* belonging to each *Fundamental Idea*. (I. 6.)

## XVIII.

The Fundamental Ideas which it is most important to consider, as being the Bases of the Material Sciences, are the Ideas of *Space*, *Time* (including Number), *Cause* (including Force and Matter), *Outness* of Objects, and *Media* of Perception of Secondary Qualities, *Polarity*

(Contrariety), Chemical *Composition* and *Affinity*, *Substance*, *Likeness* and Natural *Affinity*, *Means* and *Ends* (whence the notion of Organization), *Symmetry*, and the Ideas of *Vital Powers*. (I. 8.)

## XIX.

The Sciences which depend upon the Ideas of Space and Number are *Pure Sciences*, not *Inductive Sciences*: they do not infer special Theories from Facts, but deduce the conditions of all theory from Ideas. The Elementary Pure Sciences, or Elementary Mathematics, are Geometry, Theoretical Arithmetic and Algebra. (II. 1.)

## XX.

The Ideas on which the Pure Sciences depend, are those of *Space* and *Number*; but Number is a modification of the conception of Repetition, which belongs to the Idea of *Time*. (II. 1.)

## XXI.

The *Idea of Space* is not derived from experience, for experience of external objects *presupposes* bodies to exist in Space. Space is a condition under which the mind receives the impressions of sense, and therefore the relations of space are necessarily and universally true of all perceived objects. Space is a *form* of our perceptions, and regulates them, whatever the *matter* of them may be. (II. 2.)

## XXII.

Space is not a General Notion collected by abstraction from particular cases; for we do not speak of *Spaces* in general, but of universal or absolute *Space*. Absolute Space is infinite. All special spaces are *in* absolute space, and are parts of it. (II. 3.)

## XXIII.

Space is not a real object or thing, distinct from the objects which exist in it; but it is a real condition of the existence of external objects. (II. 3.)

## XXIV.

We have an *Intuition* of objects in space; that is, we contemplate objects as *made up* of spatial parts, and apprehend their spatial relations by the same act by which we apprehend the objects themselves. (II. 3.)

## XXV.

*Form* or *Figure* is space limited by boundaries. Space has necessarily *three* dimensions, length, breadth, depth; and no others which cannot be resolved into these. (II. 3.)

## XXVI.

The Idea of Space is exhibited for scientific purposes, by the *Definitions* and *Axioms* of Geometry; such, for instance, as these:—the *Definition of a Right Angle*, and of a *Circle*;—the *Definition of Parallel Lines*, and the *Axiom* concerning them;—the *Axiom* that *two straight lines cannot inclose a space*. These Definitions are necessary, not arbitrary; and the Axioms are needed as well as the Definitions, in order to express the necessary conditions which the Idea of Space imposes. (II. 4.)

## XXVII.

The Definitions and Axioms of Elementary Geometry do not *completely* exhibit the Idea of Space. In proceeding to the Higher Geometry, we may introduce other additional and independent Axioms; such as that of Archimedes, that *a curve line which joins two points is less than any broken line joining the same points and including the curve line*. (II. 4.)



## XXVIII.

The perception of a *solid object* by sight requires that *act* of mind by which, from figure and shade, we infer distance and position in space. The perception of *figure* by sight requires that act of mind by which we give an outline to each object. (II. 6.)

## XXIX.

The perception of Form by touch is not an impression on the passive sense, but requires an *act* of our muscular frame by which we become aware of the position of our own limbs. The perceptive faculty involved in this act has been called *the muscular sense*. (II. 6.)

## XXX.

The *Idea of Time* is not derived from experience, for experience of changes *presupposes* occurrences to take place in Time. Time is a condition under which the mind receives the impressions of sense, and therefore the relations of time are necessarily and universally true of all perceived occurrences. Time is a *form* of our perceptions, and regulates them, whatever the *matter* of them may be. (II. 7.)

## XXXI.

Time is not a General Notion collected by abstraction from particular cases. For we do not speak of particular *Times* as examples of time in general, but as parts of a single and infinite *Time*. (II. 8.)

## XXXII.

Time, like Space, is a form, not only of perception, but of *Intuition*. We consider the whole of any time as *equal* to the *sum* of the parts; and an occurrence as *coinciding* with the portion of time which it occupies. (II. 8.)

## XXXIII.

Time is analogous to Space of *one dimension* : portions of both have a beginning and an end, are long or short. There is nothing in Time which is analogous to Space of two, or of three, dimensions, and thus nothing which corresponds to Figure. (II. 8.)

## XXXIV. .

The Repetition of a set of occurrences, as, for example, strong and weak, or long and short sounds, according to a steadfast order, produces *Rhythm*, which is a conception peculiar to Time, as Figure is to Space. (II. 8.)

## XXXV.

The simplest form of Repetition is that in which there is no variety, and thus gives rise to the conception of *Number*. (II. 8.)

## XXXVI.

The simplest numerical truths are seen by Intuition ; when we endeavour to deduce the more complex from these simplest, we employ such maxims as these:—*If equals be added to equals the wholes are equal*:—*If equals be subtracted from equals the remainders are equal*:—*The whole is equal to the sum of all its parts*. (II. 9.)

## XXXVII.

The Perception of Time involves a constant and latent kind of memory, which may be termed a *Sense of Succession*. The Perception of Number also involves this Sense of Succession, although in small numbers we appear to apprehend the units simultaneously and not successively. (II. 10.)

## XXXVIII.

The Perception of Rhythm is not an impression on the passive sense, but requires an *act* of thought by which we connect and group the strokes which form the Rhythm. (II. 10.)

## XXXIX.

*Intuitive* is opposed to *Discursive* reason. In intuition, we obtain our conclusions by dwelling upon *one* aspect of the fundamental Idea; in discursive reasoning, we combine *several* aspects of the Idea, (that is, several axioms,) and reason from the combination. (II. 11.)

## XL.

Geometrical deduction (and deduction in general) is called *Synthesis*, because we introduce, at successive steps, the results of new principles. But in reasoning on the relations of space, we sometimes go on *separating* truths into their component truths, and these into other component truths; and so on; and this is geometrical *Analysis*. (II. 11.)

## XLI.

Among the foundations of the Higher Mathematics, is the *Idea of Symbols* considered as general *Signs* of Quantity. This idea of a Sign is distinct from, and independent of other ideas. The Axiom to which we refer in reasoning by means of Symbols of quantity is this:—*The interpretation of such symbols must be perfectly general.* This Idea and Axiom are the bases of Algebra in its most general form. (II. 12.)

## XLII.

Among the foundations of the Higher Mathematics is also the *Idea of a Limit*. The Idea of a Limit cannot be superseded by any other definitions or Hypotheses.

The Axiom which we employ in introducing this Idea into our reasoning is this:—*What is true up to the Limit is true at the Limit.* This Idea and Axiom are the bases of all Methods of Limits, Fluxions, Differentials, Variations, and the like. (II. 12.)

#### XLIII.

There is a *pure* Science of Motion, which does not depend upon observed facts, but upon the Idea of motion. It may also be termed *Pure Mechanism*, in opposition to Mechanics Proper, or *Machinery*, which involves the mechanical conceptions of force and matter. It has been proposed to name this Pure Science of Motion, *Kinematics*. (II. 13.)

#### XLIV.

The pure Mathematical Sciences must be successfully cultivated, in order that the progress of the principal Inductive Sciences may take place. This appears in the case of Astronomy, in which Science, both in ancient and in modern times, each advance of the theory has depended upon the previous solution of problems in pure mathematics. It appears also inversely in the Science of the Tides, in which, at present, we cannot advance in the theory, because we cannot solve the requisite problems in the Integral Calculus. (II. 14.)

#### XLV.

The *Idea of Cause*, modified into the conceptions of mechanical cause, or Force, and resistance to force, or Matter, is the foundation of the Mechanical Sciences; that is, Mechanics, (including Statics and Dynamics,) Hydrostatics, and Physical Astronomy. (III. 1.)

#### XLVI.

The Idea of Cause is not derived from experience; for in judging of occurrences which we contemplate, we

consider them as being, universally and necessarily, Causes and Effects, which a finite experience could not authorize us to do. The Axiom, that every event must have a cause, is true independently of experience, and beyond the limits of experience. (III. 2.)

#### XLVII.

The Idea of Cause is expressed for purposes of science by these three Axioms:—*Every Event must have a Cause:—Causes are measured by their Effects:—Reaction is equal and opposite to Action.* (III. 4.)

#### XLVIII.

The Conception of Force involves the Idea of Cause, as applied to the motion and rest of bodies. The conception of *force* is suggested by muscular action exerted: the conception of *matter* arises from muscular action resisted. We necessarily ascribe to all bodies solidity and inertia, since we conceive Matter as that which cannot be compressed or moved without resistance (III. 5.)

#### XLIX.

Mechanical Science depends on the Conception of Force; and is divided into *Statics*, the doctrine of Force preventing motion, and *Dynamics*, the doctrine of Force producing motion. (III. 6.)

#### L.

The Science of Statics depends upon the Axiom, that Action and Reaction are equal, which in Statics assumes this form:—*When two equal weights are supported on the middle point between them, the pressure on the fulcrum is equal to the sum of the weights.* (III. 6.)

#### LI.

The Science of Hydrostatics depends upon the Fundamental Principle that *fluids press equally in all di-*

*rections.* This principle necessarily results from the conception of a Fluid, as a body of which the parts are perfectly moveable in all directions. For since the Fluid is a body, it can transmit pressure; and the transmitted pressure is equal to the original pressure, in virtue of the Axiom that Reaction is equal to Action. That the Fundamental Principle is not derived from experience, is plain both from its evidence and from its history. (III. 6.)

## LII.

The Science of Dynamics depends upon the three Axioms above stated respecting Cause. The First Axiom,—that every change must have a Cause,—gives rise to the First Law of Motion,—that *a body not acted upon by a force will move with a uniform velocity in a straight line.* The Second Axiom,—that Causes are measured by their Effects,—gives rise to the Second Law of Motion,—that *when a force acts upon a body in motion, the effect of the force is compounded with the previously existing motion.* The Third Axiom,—that *Reaction is equal and opposite to Action*,—gives rise to the Third Law of Motion, which is expressed in the same terms as the Axiom; Action and Reaction being understood to signify momentum gained and lost (III. 7).

## LIII.

The above Laws of Motion, historically speaking, were established by means of experiment: but since they have been discovered and reduced to their simplest form, they have been considered by many philosophers as self-evident. This result is principally due to the introduction and establishment of terms and definitions, which enable us to express the Laws in a very simple manner. (III. 7.)

## LIV.

In the establishment of the Laws of Motion, it happened, in several instances, that Principles were assumed as self-evident which do not now appear evident, but which have since been demonstrated from the simplest and most evident principles. Thus it was assumed that *a perpetual motion is impossible*;—that *the velocities of bodies acquired by falling down planes or curves of the same vertical height are equal*;—that *the actual descent of the center of gravity is equal to its potential ascent*. But we are not hence to suppose that these assumptions were made without ground: for since they really follow from the laws of motion, they were probably, in the minds of the discoverers, the results of undeveloped demonstrations which their sagacity led them to divine. (III. 7.)

## LV.

It is a *Paradox* that Experience should lead us to truths confessedly universal, and apparently necessary, such as the Laws of Motion are. The *Solution* of this paradox is, that these laws are interpretations of the Axioms of Causation. The axioms are universally and necessarily true, but the right interpretation of the terms which they involve, is learnt by experience. Our Idea of Cause supplies the *Form*, Experience, the *Matter*, of these Laws. (III. 8.)

## LVI.

*Primary* Qualities of Bodies are those which we can conceive as directly perceived; *Secondary* Qualities are those which we conceive as perceived by means of a Medium. (IV. 1.)

## LVII.

We necessarily perceive bodies as *without* us: the Idea of *Externality* is one of the conditions of perception. (IV. 1.)

## LVIII.

We necessarily assume a *Medium* for the perceptions of Light, Colour, Sound, Heat, Odours, Tastes; and this Medium *must* convey impressions by means of its mechanical attributes. (IV. 1.)

## LIX.

Secondary Qualities are not *extended* but *intensive*; their effects are not augmented by addition of parts, but by increased operation of the medium. Hence they are not measured directly, but by *scales*; not by *units*, but by *degrees*. (IV. 4.)

## LX.

In the Scales of Secondary Qualities, it is a condition (in order that the scale may be complete,) that every example of the quality must either *agree* with one of the degrees of the Scale, or lie between two *contiguous* degrees. (IV. 4.)

## LXI.

We perceive *by means of* a medium and *by means of* impressions on the nerves: but we do not (by our senses,) perceive either the medium or the impressions on the nerves. (IV. 1.)

## LXII.

The *Prerogatives of the Sight* are, that by this sense we necessarily and immediately apprehend the *position* of its objects: and that from visible circumstances, we *infer* the *distance* of objects from us, so readily that we seem to perceive and not to infer. (IV. 2.)



## LXIII.

The *Prerogatives of the Hearing* are, that by this sense we perceive relations perfectly precise and definite between two notes, namely, *Musical Intervals* (as an *Octave*, a *Fifth*); and that when two notes are perceived together, they are apprehended as distinct, (a *Chord*.) and as having a certain relation, (*Concord* or *Discord*.) (IV. 2.)

## LXIV.

The Sight cannot decompose a compound colour into simple colours, or distinguish a compound from a simple colour. The Hearing cannot directly perceive the place, still less the distance, of its objects. We infer these obscurely and vaguely from audible circumstances. (IV. 2.)

## LXV.

The *First Paradox of Vision* is, that we see objects *upright*, though the images on the retina are *inverted*. The solution is, that we do not see the image on the retina at all, we only see by means of it. (IV. 2.)

## LXVI.

The *Second Paradox of Vision* is, that we see objects *single*, though there are two images on the retinas, one in each eye. The explanation is, that it is a Law of Vision that we see (small or distant) objects single, when their images fall on *corresponding points* of the two retinas. (IV. 2.)

## LXVII.

The law of single vision for *near* objects is this:—When the two images in the two eyes are situated; part for part, nearly but not exactly, upon corresponding

points, the object is apprehended as single and solid if the two objects are such as would be produced by a single solid object seen by the eyes separately. (iv. 2.)

## LXVIII.

The ultimate object of each of the Secondary Mechanical Sciences is, to determine the nature and laws of the processes by which the impression of the Secondary Quality treated of is conveyed: but before we discover the *cause*, it may be necessary to determine the *laws* of the phenomena; and for this purpose a *Measure* or *Scale* of each quality is necessary. (iv. 4.)

## LXIX.

Secondary qualities are measured by means of such effects as can be estimated in number or space. (iv. 4.)

## LXX.

The Measure of Sounds, as high or low, is the *Musical Scale*, or *Harmonic Canon*. (iv. 4.)

## LXXI.

The Measures of Pure Colours are the *Prismatic Scale*; the same, including *Fraunhofer's Lines*; and *Newton's Scale* of Colours. The principal Scales of Impure Colours are *Werner's Nomenclature* of Colours, and *Merimée's Nomenclature* of Colours. (iv. 4.)

## LXXII.

The Idea of *Polarity* involves the conception of contrary properties in contrary directions:—the properties being, for example, attraction and repulsion, darkness and light, synthesis and analysis; and the contrary directions being those which are directly opposite, or, in some cases, those which are at right angles. (v. 1.)

LXXIII. (*Doubtful.*)

Coexistent polarities are fundamentally identical.  
(v. 2.)

## LXXIV.

The Idea of Chemical *Affinity*, as implied in Elementary Composition, involves peculiar conceptions. It is not properly expressed by assuming the qualities of bodies to *resemble* those of the elements, or to depend on the *figure* of the elements, or on their *attractions*.  
(vi. 1.)

## LXXV.

Attractions take place between bodies, Affinities between the particles of a body. The former may be compared to the alliances of states, the latter to the ties of family. (vi. 2.)

## LXXVI.

The governing principles of Chemical Affinity are, that it is *elective*; that it is *definite*; that it *determines the properties* of the compound; and that *analysis is possible*. (vi. 2.)

## LXXVII.

We have an idea of *Substance*: and an axiom involved in this Idea is, that *the weight of a body is the sum of the weights of all its elements*. (vi. 3.)

## LXXVIII.

Hence Imponderable Fluids are not to be admitted as chemical elements. (vi. 4.)

## LXXIX.

The Doctrine of Atoms is admissible as a mode of expressing and calculating laws of nature; but is not proved by any fact, chemical or physical, as a philosophical truth. (vi. 5.)

## LXXX.

We have an Idea of *Symmetry*; and an axiom involved in this Idea is, that a symmetrical natural body, if there be a tendency to modify any member in any manner, there is a tendency to modify all the corresponding members in the same manner. (VII. 1.)

## LXXXI.

All hypotheses respecting the manner in which the elements of inorganic bodies are arranged in space, must be constructed with regard to the general facts of crystallization. (VII. 3.)

## LXXXII.

When we consider any object as *One*, we give unity to it by an act of thought. The condition which determines what this unity shall include, and what it shall exclude, is this;—that assertions concerning the one thing shall be possible. (VIII. 1.)

## LXXXIII.

We collect individuals into *Kinds* by applying to them the Idea of Likeness. Kinds of things are not determined by definitions, but by this condition;—that general assertions concerning such kinds of things shall be possible. (VIII. 1.)

## LXXXIV.

The *Names* of kinds of things are governed by their use; and that may be a right name in one use which is not so in another. A whale is not a *fish* in natural history, but it is a *fish* in commerce and law. (VIII. 1.)

## LXXXV.

We take for granted that each kind of things has a special *character* which may be expressed by a Defini-

tion. The ground of our assumption is this;—that reasoning must be possible. (VIII. 1.)

## LXXXVI.

The “Five Words,” *Genus, Species, Difference, Property, Accident*, were used by the Aristotelians, in order to express the subordination of kinds, and to describe the nature of definitions and propositions. In modern times, these technical expressions have been more referred to by Natural Historians than by Metaphysicians. (VIII. 1.)

## LXXXVII.

The construction of a Classificatory Science includes *Terminology*, the formation of a descriptive language;—*Diataxis*, the Plan of the System of Classification, called also the *Systematick*;—*Diagnosis*, the Scheme of the Characters by which the different Classes are known, called also the *Characteristick*. *Physiography* is the knowledge which the System is employed to convey. *Diataxis* includes *Nomenclature*. (VIII. 2.)

## LXXXVIII.

*Terminology* must be conventional, precise, constant; copious in words, and minute in distinctions, according to the needs of the science. The student must understand the terms, *directly* according to the convention, not through the medium of explanation or comparison. (VIII. 2.)

## LXXXIX.

The *Diataxis*, or Plan of the System, may aim at a Natural or an Artificial System. But no classes can be absolutely artificial, for if they were, no assertions could be made concerning them. (VIII. 2.)

## XC.

An *Artificial System* is one in which the *smaller* groups (the Genera) are *natural*; and in which the *wider* divisions (Classes, Orders) are constructed by the *peremptory* application of selected Characters; (selected, however, so as not to break up the smaller groups.) (VIII. 2.)

## XCI.

A *Natural System* is one which attempts to make *all* the divisions *natural*, the widest as well as the narrowest; and therefore applies *no* characters *peremptorily*. (VIII. 2.)

## XCII.

Natural Groups are best described, not by any definition which marks their boundaries, but by a *Type* which marks their center. The Type of any natural group is an example which possesses in a marked degree all the leading characters of the class. (VIII. 2.)

## XCIII.

A Natural Group is steadily fixed, though not precisely limited; it is given in position, though not circumscribed; it is determined, not by a boundary without, but by a central point within;—not by what it strictly excludes, but by what it eminently includes;—by a Type, not by a Definition. (VIII. 2.)

## XCIV.

The prevalence of Mathematics as an element of education has made us think Definition the philosophical mode of fixing the meaning of a word: if (Scientific) Natural History were introduced into education, men might become familiar with the fixation of the signifi-

cation of words by Types; and this process agrees more nearly with the common processes by which words acquire their significations. (VIII. 2.)

## XCV.

The attempts at Natural Classification are of three sorts; according as they are made by the process of *blind trial*, of *general comparison*, or of *subordination of characters*. The process of Blind Trial professes to make its classes by attention to *all* the characters, but without proceeding methodically. The process of General Comparison professes to enumerate all the characters, and forms its classes by the *majority*. Neither of these methods can really be carried into effect. The method of Subordination of Characters considers some characters as *more important* than others; and this method gives more consistent results than the others. This method, however, does not depend upon the Idea of Likeness only, but introduces the Idea of Organization or Function. (VIII. 2.)

## XCVI.

A *Species* is a collection of individuals which are descended from a common stock, or which resemble such a collection as much as these resemble each other: the resemblance being opposed to a *definite* difference. (VIII. 2.)

## XCVII.

A *Genus* is a collection of species which resemble each other more than they resemble other species: the resemblance being opposed to a *definite* difference. (VIII. 2.)

## XCVIII.

The *Nomenclature* of a Classificatory Science is the collection of the names of the Species, Genera, and other divisions. The *binary* nomenclature, which denotes a species by the *generic* and *specific* name, is now commonly adopted in Natural History. (VIII. 2.)

## XCIX.

The *Diagnosis*, or Scheme of the Characters, comes, in the order of philosophy, after the Classification. The characters do not *make* the classes, they only enable us to *recognize* them. The Diagnosis is an Artificial Key to a Natural System. (VIII. 2.)

## C.

The basis of all Natural Systems of Classification is the Idea of Natural Affinity. The Principle which this Idea involves is this:—Natural arrangements, obtained from *different* sets of characters, must *coincide* with each other. (VIII. 4.)

## CI.

In order to obtain a Science of Biology, we must analyze the Idea of Life. It has been proved by the biological speculations of past time, that Organic Life cannot rightly be solved into Mechanical or Chemical Forces, or the operation of a Vital Fluid, or of a Soul. (IX. 2.)

## CII.

Life is a System of Vital Forces; and the conception of such Forces involves a peculiar Fundamental Idea. (IX. 3.)

## CIII.

Mechanical, chemical, and vital Forces form an ascending progression, each including the preceding. Chemical Affinity includes in its nature Mechanical Force, and may often be practically resolved into Mechanical Force. (Thus the ingredients of gunpowder, liberated from their chemical union, exert great mechanical Force: a galvanic battery acting by chemical process does the like.) Vital Forces include in their nature both chemical Affinities and mechanical Forces: for



Vital Powers produce both chemical changes, (as digestion,) and motions which imply considerable mechanical force, (as the motion of the sap and of the blood. (IX. 4.)

## CIV.

In *voluntary* motions, Sensations produce Actions, and the connexion is made by means of Ideas: in *reflected* motions, the connexion neither seems to be nor is made by means of Ideas: in *instinctive* motions, the connexion is such as requires Ideas, but we cannot believe the Ideas to exist. (IX. 5.)

## CV.

The assumption of a Final Cause in the structure of each part of animals and plants is as inevitable as the assumption of an Efficient Cause for every event. The maxim that in organized bodies nothing is *in vain*, is as necessarily true as the maxim that nothing happens *by chance*. (IX. 6.)

## CVI.

The idea of living beings as subject to *disease* includes a recognition of a Final Cause in organization; for disease is a state in which the vital forces do not attain their *proper ends*. (IX. 6.)

## CVII.

The Palætiological Sciences depend upon the Idea of Cause; but the leading conception which they involve is that of *historical cause*, not mechanical cause. (X. 1.)

## CVIII.

Each Palætiological Science, when complete, must possess three members: the *Phenomenology*, the *Ætiology*, and the *Theory*. (X. 2.)

## CIX.

There are, in the Palætiological Sciences, two antagonist doctrines: *Catastrophes* and *Uniformity*. The doctrine of a *uniform course of nature* is tenable only when we extend the notion of Uniformity so far that it shall include *Catastrophes*. (x. 3.)

## CX.

The Catastrophist constructs Theories, the Uniformitarian demolishes them. The former adduces evidence of an Origin, the latter explains the evidence away. The Catastrophist's dogmatism is undermined by the Uniformitarian's skeptical hypotheses. But when these hypotheses are asserted dogmatically, they cease to be consistent with the doctrine of Uniformity. (x. 3.)

## CXI.

In each of the Palætiological Sciences, we can ascend to remote periods by a chain of causes, but in none can we ascend to a *beginning* of the chain. (x. 3.)

## CXII.

In contemplating the series of causes and effects which constitutes the world, we necessarily assume a *First Cause* of the whole series. (x. 5.)

## CXIII.

The Palætiological Sciences point backwards with lines which are broken, but which all converge to the *same* invisible point: and this point is the Origin of the Moral and Spiritual, as well as of the Natural World. (x. 5.)

## APHORISMS CONCERNING SCIENCE.

## I.

THE two processes by which Science is constructed are the *Explication of Conceptions* and the *Colligation of Facts*. (Book XI. Chap. 1.)

## II.

The Explication of Conceptions, as requisite for the progress of science, has been effected by means of discussions and controversies among scientists; often by debates concerning definitions; these controversies have frequently led to the establishment of a Definition; but along with the Definition, a corresponding Proposition has always been expressed or implied. The essential requisite for the advance of science is the clearness of the Conception, not the establishment of a Definition. The construction of an exact Definition is often very difficult. The requisite conditions of clear Conceptions may often be expressed by Axioms as well as by Definitions. (XI. 2.)

## III.

Conceptions, for purposes of science, must be *appropriate* as well as clear: that is, they must be modifications of that Fundamental Idea, by which the phenomena can really be interpreted. This maxim may warn us from error, though it may not lead to discovery. Discovery depends upon the previous cultivation or natural clearness of the appropriate Idea, and therefore *no discovery is the work of accident*. (XI. 2.)

## IV.

Facts are the materials of science, but all Facts involve Ideas. Since, in observing Facts, we cannot exclude Ideas, we must, for the purposes of science, take care that the Ideas are clear and rigorously applied. (XI. 3.)

## V.

The last Aphorism leads to such Rules as the following:—That Facts, for the purposes of material science, must involve Conceptions of the Intellect only, and not Emotions:—That Facts must be observed with reference to our most exact conceptions, Number, Place, Figure, Motion:—That they must also be observed with reference to any other exact conceptions which the phenomena suggest, as Force, in mechanical phenomena, Concord, in musical. (XI. 3.)

## VI.

The resolution of complex Facts into precise and measured partial Facts, we call the *Decomposition of Facts*. This process is requisite for the progress of science, but does not necessarily lead to progress. (XI. 3.)

## VII.

Science begins with *common* observation of facts; but even at this stage, requires that the observations be precise. Hence the sciences which depend upon space and number were the earliest formed. After common observation, come Scientific *Observation* and *Experiment*. (XI. 4.)

## VIII.

The Conceptions by which Facts are bound together, are suggested by the sagacity of discoverers. This sagacity cannot be taught. It commonly succeeds by guess-

ing; and this success seems to consist in framing several *tentative hypotheses* and selecting the right one. But a supply of appropriate hypotheses cannot be constructed by rule, nor without inventive talent. (XI. 4.)

## IX.

The truth of tentative hypotheses must be tested by their application to facts. The discoverer must be ready, carefully to try his hypotheses in this manner, and to reject them if they will not bear the test, in spite of indolence and vanity. (XI. 4.)

## X.

The process of scientific discovery is cautious and rigorous, not by abstaining from hypotheses, but by rigorously comparing hypotheses with facts, and by resolutely rejecting all which the comparison does not confirm. (XI. 5.)

## XI.

Hypotheses may be useful, though involving much that is superfluous, and even erroneous: for they may supply the true bond of connexion of the facts; and the superfluity and error may afterwards be pared away. (XI. 5.)

## XII.

It is a test of true theories not only to account for, but to predict phenomena. (XI. 5.)

## XIII.

*Induction* is a term applied to describe the *process* of a true Colligation of Facts by means of an exact and appropriate Conception. *An Induction* is also employed to denote the *proposition* which results from this process. (XI. 5.)

## XIV.

*The Consilience of Inductions* takes place when an Induction, obtained from one class of facts, coincides with an Induction, obtained from another different class. This Consilience is a test of the truth of the Theory in which it occurs. (XI. 5.)

## XV.

An Induction is not the mere *sum* of the Facts which are colligated. The Facts are not only brought together, but seen in a new point of view. A new mental Element is *superinduced*; and a peculiar constitution and discipline of mind are requisite in order to make this Induction. (XI. 5.)

## XVI.

Although in Every Induction a new conception is superinduced upon the Facts; yet this once effectually done, the novelty of the conception is overlooked, and the conception is considered as a part of the fact. (XI. 5.)

## XVII.

The *Logic of Induction* consists in stating the Facts and the Inference in such a manner, that the Evidence of the Inference is manifest; just as the Logic of Deduction consists in stating the Premises and the Conclusion in such a manner that the Evidence of the Conclusion is manifest. (XI. 6.)

## XVIII.

The Logic of Deduction is exhibited by means of a certain Formula; namely, a Syllogism; and every train of deductive reasoning, to be demonstrative, must be capable of resolution into a series of such Formulæ legitimately constructed. In like manner, the Logic of Induction may be exhibited by means of certain *Formulæ*;

and every train of inductive inference, to be sound, must be capable of resolution into a scheme of such Formulæ, legitimately constructed. (XI. 6.)

## XIX.

The *inductive act of thought* by which several Facts are colligated into one Proposition, may be expressed by saying: *The several Facts are exactly expressed as one Fact, if, and only if, we adopt the Conceptions and the Assertion of the Proposition.* (XI. 6.)

## XX.

The One Fact, thus inductively obtained from several Facts, may be combined with other Facts, and colligated with them by a new act of Induction. This process may be indefinitely repeated: and these successive processes are the *Steps of Induction*, or of *Generalization*, from the lowest to the highest. (XI. 6.)

## XXI.

The relation of the successive Steps of Induction may be exhibited by means of an *Inductive Table*, in which the several Facts are indicated, and tied together by a Bracket, and the Inductive Inference placed on the other side of the Bracket; and this arrangement repeated, so as to form a genealogical Table of each Induction, from the lowest to the highest. (XI. 6.)

## XXII.

The Logic of Induction is the *Criterion of Truth* inferred from Facts, as the Logic of Deduction is the Criterion of Truth deduced from necessary Principles. The Inductive Table enables us to apply such a Criterion; for we can determine whether each Induction is verified and justified by the Facts which its Bracket

includes; and if each induction in particular be sound, the highest, which merely combines them all, must necessarily be sound also. (XI. 6.)

## XXIII.

The distinction of *Fact* and *Theory* is only relative. Events and phenomena, considered as particulars which may be colligated by Induction, are *Facts*; considered as generalities already obtained by colligation of other Facts, they are *Theories*. The same event or phenomenon is a Fact or a Theory, according as it is considered as standing on one side or the other of the Inductive Bracket. (XI. 6.)

## XXIV.

Inductive truths are of two kinds, *Laws of Phenomena*, and *Theories of Causes*. It is necessary to begin in every science with the Laws of Phenomena; but it is impossible that we should be satisfied to stop short of a Theory of Causes. In Physical Astronomy, Physical Optics, Geology, and other sciences, we have instances showing that we can make a great advance in inquiries after true Theories of Causes. (XI. 7.)

## XXV.

Art and Science differ. The object of Science is Knowledge; the objects of Art, are Works. In Art, truth is a means to an end; in Science, it is the only end. Hence the Practical Arts are not to be classed among the Sciences. (XI. 8.)

## XXVI.

Practical Knowledge, such as Art implies, is not Knowledge such as Science includes. Brute animals have a practical knowledge of relations of space and force; but they have no knowledge of Geometry or Mechanics. (XI. 8.)



## XXVII.

The Methods by which the constructions of Science is promoted are, *Methods of Observation*, *Methods of obtaining clear Ideas*, and *Methods of Induction*. (XII. 1.)

## XXVIII.

The Methods of Observation of Quantity in general are, *Numeration*, which is precise by the nature of Number; the *Measurement of Space* and of *Time*, which are easily made precise; the *Conversion of Space and Time*, by which each aids the measurement of the other; the *Method of Repetition*; the *Method of Coincidences or Interferences*. The measurement of Weight is made precise by the *Method of Double-weighing*. Secondary Qualities are measured by means of *Scales of Degrees*; but in order to apply these Scales, the student requires the *Education of the Senses*. The Education of the Senses is forwarded by the practical study of *Descriptive Natural History*, *Chemical Manipulation*, and *Astronomical Observation*. (XII. 2.)

## XXIX.

The Methods by which the acquisition of clear Scientific Ideas is promoted, are mainly two; *Intellectual Education* and *Discussion of Ideas*. (XII. 3.)

## XXX.

The Idea of Space becomes more clear by studying *Geometry*; the Idea of Force, by studying *Mechanics*; the Ideas of Likeness, of Kind, of Subordination of Classes, by studying *Natural History*. (XII. 3.)

## XXXI.

*Elementary Mechanics* should now form a part of intellectual education, in order that the student may

understand the Theory of Universal Gravitation: for an intellectual education should cultivate such ideas as enable the student to understand the most complete and admirable portions of the knowledge which the human race has attained to. (XII. 3.)

## XXXII.

*Natural History* ought to form a part of intellectual education, in order to correct certain prejudices which arise from cultivating the intellect by means of mathematics alone; and in order to lead the student to see that the division of things into Kinds, and the attribution and use of Names, are processes susceptible of great precision. (XII. 3.)

## XXXIII.

The conceptions involved in scientific truths have attained the requisite degree of clearness by means of the *Discussions* respecting ideas which have taken place among discoverers and their followers. Such discussions are very far from being unprofitable to science. They are metaphysical, and must be so: the difference between discoverers and barren reasoners is, that the former employ good, and the latter bad metaphysics. (XII. 4.)

## XXXIV.

The Process of Induction may be resolved into three steps; the *Selection of the Idea*, the *Construction of the Conception*, and the *Determination of the Magnitudes*. (XII. 5.)

## XXXV.

These three steps correspond to the determination of the *Independent Variable*, the *Formula*, and the *Coefficients*, in mathematical investigations; or to the *Argu-*

*ment*, the *Law*, and the *Numerical Data*, in a Table of an Inequality. (XII. 5.)

## XXXVI.

The Selection of the Idea depends mainly upon inventive sagacity: which operates by suggesting and trying various hypotheses. Some inquirers try erroneous hypotheses; and thus, exhausting the forms of error, form the Prelude to Discovery. (XII. 5.)

## XXXVII.

The following Rules may be given, in order to the selection of the Idea for purposes of Induction:—the Idea and the Facts must be *homogeneous*; and the Rule must be *tested by the Facts*. (XII. 5.)

## XXXVIII.

The Construction of the Conception very often includes, in a great measure, the Determination of the Magnitudes. (XII. 6.)

## XXXIX.

When a series of *progressive* numbers is given as the result of observation, it may generally be reduced to law by combinations of arithmetical and geometrical progressions. (XII. 6.)

## XL.

A true formula for a progressive series of numbers cannot commonly be obtained from a *narrow range* of observations. (XII. 6.)

## XLI.

*Recurrent* series of numbers must, in most cases, be expressed by circular formulæ. (XII. 6.)

## XLII.

The true construction of the conception is frequently suggested by some hypothesis; and in these cases, the

hypothesis may be useful, though containing superfluous parts. (XII. 6.)

## XLIII.

There are special Methods of Induction applicable to Quantity; of which the principal are, the *Method of Curves*, the *Method of Means*, the *Method of Least Squares*, and the *Method of Residues*. (XII. 7.)

## XLIV.

*The Method of Curves* consists in drawing a curve, of which the observed quantities are the ordinates, the quantity on which the change of these quantities depends being the abscissa. The efficacy of this Method depends upon the faculty which the eye possesses, of readily detecting regularity and irregularity in forms. The Method may be used to detect the laws which the observed quantities follow; and also, when the observations are inexact, it may be used to correct these observations, so as to obtain data more true than the observed facts themselves. (XII. 7.)

## XLV.

*The Method of Means* gets rid of irregularities by taking the arithmetical mean of a great number of observed quantities. Its efficacy depends upon this; that in cases in which observed quantities are affected by other inequalities, besides that of which we wish to determine the law, the excesses *above* and defects *below* the quantities which the law in question would produce, will, in a collection of many observations, *balance* each other. (XII. 7.)

## XLVI.

*The Method of Least Squares* is a Method of Means, in which the mean is taken according to the condition, that the sum of the squares of the errors of observa-

tion shall be the least possible which the law of the facts allows. It appears, by the doctrine of chances, that this is the *most probable* mean. (XII. 7.)

#### XLVII.

*The Method of Residues* consists in subtracting, from the quantities given by observation, the quantity given by any law already discovered; and then examining the remainder, or *Residue*, in order to discover the leading law which it follows. When this second law has been discovered, the quantity given by it may be subtracted from the first *Residue*; thus giving a *Second Residue*, which may be examined in the same manner; and so on. The efficacy of this Method depends principally upon the circumstance of the laws of variation being successively smaller and smaller in amount (or at least in their mean effect); so that the ulterior undiscovered laws do not prevent the law in question from being *prominent* in the observations. (XII. 7.)

#### XLVIII.

The Method of Means and the Method of Least Squares cannot be applied without our *knowing the Arguments* of the Inequalities which we seek. The Method of Curves and the Method of Residues, when the Arguments of the principal Inequalities are known, often make it easy to find the others. (XII. 7.)

#### XLIX.

*The Law of Continuity* is this:—that a quantity cannot pass from one amount to another by any change of conditions, without passing through all intermediate magnitudes according to the intermediate conditions. This Law may often be employed to disprove distinctions which have no real foundation. (XII. 8.)

## L.

*The Method of Gradation* consists in taking a number of stages of a property in question, intermediate between two extreme cases which appear to be different. This Method is employed to determine whether the extreme cases are really distinct or not. (XII. 8.)

## LI.

The Method of Gradation, applied to decide the question, whether the existing geological phenomena arise from existing causes, leads to this result:—That the phenomena do appear to arise from existing causes, but that the action of existing causes may, in past times, have transgressed, to any extent, their recorded limits of intensity. (XII. 8.)

## LII.

*The Method of Natural Classification* consists in classing cases, not according to any assumed definition, but according to the connexion of the facts themselves, so as to make them the means of asserting general truths. (XII. 8.)

## LIII.

In the *Induction of Causes* the principal Maxim is, that we must be careful to possess, and to apply, with perfect clearness, the Fundamental Idea on which the Induction depends. (XII. 10.)

## LIV.

The Induction of Substance, of Force, of Polarity, go beyond mere laws of phenomena, and may be considered as the Induction of Causes. (XII. 10.)

## LV.

The Cause of certain phenomena being inferred, we are led to inquire into the Cause of this Cause, which

inquiry must be conducted in the same manner as the previous one; and thus we have the Induction of Ulterior Causes. (xii. 10.)

#### LVI.

In contemplating the series of Causes which are themselves the effects of other causes, we are necessarily led to assume a Supreme Cause in the Order of Causation, as we assume a First Cause in Order of Succession. (xii. 10.)

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## APHORISMS

## CONCERNING THE LANGUAGE OF SCIENCE.

## INTRODUCTION.

It has been shown in the *History of Science*, and has further appeared in the course of the present work, that almost every step in the progress of science is marked by the formation or appropriation of a technical term. Common language has, in most cases, a certain degree of looseness and ambiguity; as common knowledge has usually something of vagueness and indistinctness. In common cases too, knowledge usually does not occupy the intellect alone, but more or less interests some affection, or puts in action the fancy; and common language, accommodating itself to the office of expressing such knowledge, contains, in every sentence, a tinge of emotion or of imagination. But when our knowledge becomes perfectly exact and purely intellectual, we require a language which shall also be exact and intellectual;—which shall exclude alike vagueness and fancy, imperfection and superfluity;—in which each term shall convey a meaning steadily fixed and rigorously limited. Such a language that of science becomes, through the use of Technical Terms. And we must now endeavour to lay down some maxims and suggestions, by attention to which Technical Terms may be better fitted to answer their purpose. In order to do this, we shall in the first place take a rapid survey of the manner in which Technical Terms have been employed from the earliest periods of scientific history.



The progress of the use of technical scientific language offers to our notice two different and successive periods; in the first of which, technical terms were formed casually, as convenience in each case prompted; while in the second period, technical language was constructed intentionally, with set purpose, with a regard to its connexion, and with a view of constructing a system. Though the casual and the systematic formation of technical terms cannot be separated by any precise date of time, (for at all periods some terms in some sciences have been framed unsystematically,) we may, as a general description, call the former the *Ancient* and the latter the *Modern* Period. In illustrating the two following Aphorisms, I will give examples of the course followed in each of these periods.

#### APHORISM I.

*In the Ancient Period of Science, Technical Terms were formed in three different ways:—by appropriating common words and fixing their meaning;—by constructing terms containing a description;—by constructing terms containing reference to a theory.*

THE earliest sciences offer the earliest examples of technical terms. These are Geometry, Arithmetic, and Astronomy; to which we have soon after to add Harmonics, Mechanics, and Optics. In these sciences, we may notice the above-mentioned three different modes in which technical terms were formed.

I. The simplest and first mode of acquiring technical terms, is to take words current in common usage, and by rigorously defining or otherwise fixing their meaning, to fit them for the expression of scientific truths. In this manner almost all the fundamental technical terms of Geometry were formed. A *sphere*, a *cone*, a *cylinder*,

had among the Greeks, at first, meanings less precise than those which geometers gave to these words, and besides the mere designation of form, implied some use or application. A *sphere* (σφαῖρα) was a hand-ball used in games; a *cone* (κῶνος) was a boy's spinning-top, or the crest of a helmet; a *cylinder* (κύλινδρος) was a roller; a *cube* (κύβος) was a die: till these words were adopted by the geometers, and made to signify among them pure modifications of space. So an *angle* (γωνία) was only a corner; a *point* (σημεῖον) was a signal; a *line* (γραμμὴ) was a mark; a *straight line* (εὐθεῖα) was marked by an adjective which at first meant only *direct*. A *plane* (ἐπιπεδον) is the neuter form of an adjective, which by its derivation means *on the ground*, and hence *flat*. In all these cases, the word adopted as a term of science has its sense rigorously fixed; and where the common use of the term is in any degree vague, its meaning may be modified at the same time that it is thus limited. Thus a *rhombus* (ρόμβος) by its derivation, might mean any figure which is *twisted* out of a regular form; but it is confined by geometers to that figure which has four equal sides, its angles being oblique. In like manner, a *trapezium* (τραπέζιον) originally signifies a *table*, and thus might denote any form; but as the tables of the Greeks had one side shorter than the opposite one, such a figure was at first called a *trapezium*. Afterwards the term was made to signify any figure with four unequal sides; a name being more needful in geometry for this kind of figure than for the original form.

This class of technical terms, namely, words adopted from common language, but rendered precise and determinate for purposes of science, may also be exemplified in other sciences. Thus, as was observed in the early portion of the history of astronomy\*, a *day*, a *month*,

\* *Hist. Ind. Sci.*, B. III. c. i.

a *year*, described at first portions of time marked by familiar changes, but afterwards portions determined by rigorous mathematical definitions. The conception of the heavens as a revolving sphere, is so obvious, that we may consider the terms which involve this conception as parts of common language; as the *pole* (πόλος); the *arctic circle*, which includes the stars that never set\*; the *horizon* (ὁρίζων) a boundary, applied technically to the circle bounding the visible earth and sky. The *turnings of the sun* (τροπαι ἡελίου), which are mentioned by Hesiod, gave occasion to the term *tropics*, the circles at which the sun in his annual motion turns back from his northward or southward advance. The *zones* of the earth, (the *torrid*, *temperate*, and *frigid*;) the *gnomon* of a dial; the *limb* (or border) of the moon, or of a circular instrument, are terms of the same class. An *eclipse* (ἔκλειψις) is originally a deficiency or disappearance, and joined with the name of the luminary, an *eclipse of the sun* or *of the moon*, described the phenomenon; but when the term became technical, it sufficed, without addition, to designate the phenomenon.

In Mechanics, the Greeks gave a scientific precision to very few words: we may mention *weights* (βάρειά), the *arms of a lever* (μήχεα), its *fulcrum* (ὑπομοχλίου), and the verb *to balance* (ισορροπεῖν). Other terms which they used, as *momentum* (ρόπη) and *force* (δύναμις), did not acquire a distinct and definite meaning till the time of Galileo, or later. We may observe that all abstract terms, though in their scientific application expressing mere conceptions, were probably at first derived from some word describing external objects. Thus the Latin word for force, *vis*, seems to be connected with a Greek word, ἰς, or *Fi*s, which often has nearly the same meaning; but originally, as it would seem, signified a sinew or muscle, the obvious seat of animal strength.

\* *Hist. Ast.*, B. III. c. i. sect. 8.

In later times, the limitation imposed upon a word by its appropriation to scientific purposes, is often more marked than in the cases above described. Thus the *variation* is made to mean, in astronomy, the second inequality of the moon's motion; in magnetism, the *variation* signifies the angular deviation of the compass-needle from the north; in pure mathematics, the *variation* of a quantity is the formula which expresses the result of any small change of the most general kind. In like manner, *parallax* (*παράλλαξις*) denotes a *change* in general, but is used by astronomers to signify the change produced by the spectator's being removed from the center of the earth, his theoretical place, to the surface. *Alkali* at first denoted the ashes of a particular plant, but afterwards, all bodies having a certain class of chemical properties; and, in like manner, *acid*, the class opposed to alkali, was modified in signification by chemists, so as to refer no longer to the taste.

Words thus borrowed from common language, and converted by scientific writers into technical terms, have some advantages and some disadvantages. They possess this great convenience, that they are understood after a very short explanation, and retained in the memory without effort. On the other hand, they lead to some inconvenience; for since they have a meaning in common language, a careless reader is prone to disregard the technical limitation of this meaning, and to attempt to collect their import in scientific books, in the same vague and conjectural manner in which he collects the purpose of words in common cases. Hence the language of science, when thus resembling common language, is liable to be employed with an absence of that scientific precision which alone gives it value. Popular writers and talkers, when they speak of *force*, *momentum*, *action* and *reaction*, and the like, often afford examples of the

inaccuracy thus arising from the scientific appropriation of common terms.

II. Another class of technical terms, which we find occurring as soon as speculative science assumes a distinct shape, consists of those which are intentionally constructed by speculators, and which contain some description or indication distinctive of the conception to which they are applied. Such are a *parallelogram* (παράλληλόγραμμον), which denotes a plane figure bounded by two pairs of parallel lines; a *parallelopiped* (παράλληλοπίπεδον), which signifies a solid figure bounded by three pairs of parallel planes. A *triangle* (τρίγωνος, *trigon*) and a *quadrangle* (τετράγωνος, *tetragon*) were perhaps words invented independently of the mathematicians: but such words extended to other cases, *pentagon*, *decagon*, *heccædecagon*, *polygon*, are inventions of scientific men. Such also are *tetrahedron*, *hexahedron*, *dodecahedron*, *tesseracontaoctohedron*, *polyhedron*, and the like. These words being constructed by speculative writers, explain themselves, or at least require only some conventional limitation, easily adopted. Thus *parallelogram* might mean a figure bounded by any number of sets of parallel lines, but it is conventionally restricted to a figure of *four* sides. So a *great circle* in a sphere means one which passes through the center of the sphere; and a *small circle* is any other. So in trigonometry, we have the *hypotenuse* (ὑποτενίονσα), or *subtending* line, to designate the line subtending an angle, and especially a right angle. In this branch of mathematics we have many invented technical terms; as *complement*, *supplement*, *cosine*, *cotangent*, a *spherical angle*, the *pole of a circle*, or of a sphere. The word *sine* itself appears to belong to the class of terms already described as scientific appropriations of common terms, although its origin is somewhat obscure.

Mathematicians were naturally led to construct these and many other terms by the progress of their speculations. In like manner, when astronomy took the form of a speculative science, words were invented to denote distinctly the conceptions thus introduced. Thus the sun's annual path among the stars, in which not only solar, but also all lunar eclipses occur, was termed the *ecliptic*. The circle which the sun describes in his diurnal motion, when the days and nights are equal, the Greeks called the *equidiurnal* (ισημερινός,) the Latin astronomers the *equinoctial*, and the corresponding circle on the earth was the *equator*. The ecliptic intersected the equinoctial in the *equinoctial points*. The *solstices* (in Greek, τροπαί) were the times when the sun arrested his motion northwards or southwards; and the *solstitial points* (τὰ τροπικά σημεία) were the places in the ecliptic where he then was. The name of *meridians* was given to circles passing through the poles of the equator; the *solstitial colure* (κόλουργος, curtailed), was one of these circles, which passes through the solstitial points, and is intercepted by the horizon.

We have borrowed from the Arabians various astronomical terms, as *Zenith*, *Nadir*, *Azimuth*, *Almacantar*. And these words, which among the Arabians probably belonged to the first class, of appropriated scientific terms, are for us examples of the second class, invented scientific terms; although they differ from most that we have mentioned, in not containing an etymology corresponding to their meaning in any language with which European cultivators of science are generally familiar. Indeed, the distinction of our two classes, though convenient, is in a great measure, casual. Thus most of the words we formerly mentioned, as *parallax*, *horizon*, *eclipse*, though appropriated technical terms among the Greeks, are to us invented technical terms.

In the construction of such terms as we are now considering, those languages have a great advantage which possess a power of forming words by composition. This was eminently the case with the Greek language; and hence most of the ancient terms of science in that language, when their origin is once explained, are clearly understood and easily retained. Of modern European languages, the German possesses the greatest facility of composition; and hence scientific authors in that language are able to invent terms which it is impossible to imitate in the other languages of Europe. Thus Weiss distinguishes his various systems of crystals as *zwei-und-zwei-gliedrig*, *ein-und-zwei-gliedrig*, *drey-und-drey-gliedrig*, &c., (two-and-two-membered, one-and-two-membered, three-and-three-membered.) And Hessel, also a writer on crystallography, speaks of *doubly-one-membered edges*, *four-and-three spaced rays*, and the like.

How far the composition of words, in such cases, may be practised in the English language, and the general question, what are the best rules and artifices in such cases, I shall afterwards consider. In the mean time, I may observe that this list of invented technical terms might easily be much enlarged. Thus in harmonics we have the various intervals, as a *Fourth*, a *Fifth*, an *Octave*, (*Diatessaron*, *Diapente*, *Diapason*,) a *Comma*, which is the difference of a *Major* and *Minor Tone*; we have the various *Moods* or *Keys*, and the notes of various lengths, as *Minims*, *Breves*, *Semibreves*, *Quavers*. In chemistry, *Gas* was at first a technical term invented by Van Helmont, though it has now been almost adopted into common language. I omit many words which will perhaps suggest themselves to the reader, because they belong rather to the next class, which I now proceed to notice.

III. The third class of technical terms consists of

such as are constructed by men of science, and involve some theoretical idea in the meaning which their derivation implies. They do not merely describe, like the class last spoken of, but describe with reference to some doctrine or hypothesis which is accepted as a portion of science. Thus *latitude* and *longitude*, according to their origin, signify breadth and length; they are used, however, to denote measures of the distance of a place on the earth's surface from the equator, and from the first meridian, of which distances, one cannot be called *length* more properly than the other. But this appropriation of these words may be explained by recollecting that the earth, as known to the ancient geographers, was much further extended from east to west than from north to south. The *Precession* of the equinoxes is a term which implies that the stars are fixed, while the point which is the origin of the measure of celestial longitude moves backward. The *Right Ascension* of a star is a measure of its position corresponding to terrestrial longitude; this quantity is identical with the angular ascent of the equinoctial point, when the star is in the horizon in a *right* sphere; that is, a sphere which supposes the spectator to be at the equator. The *Oblique Ascension* (a term now little used), is derived in like manner from an oblique sphere. The motion of a planet is *direct* or *retrograde*, *in consequentia* (*signa*), or *in antecedentia*, in reference to a certain assumed standard direction for celestial motions, namely, the direction opposite to that of the sun's daily motion, and agreeing with his annual motion among the stars; or with what is much more evident, the moon's monthly motion. The *equation of time* is the quantity which must be added to or subtracted from the time marked by the sun, in order to reduce it to a theoretical condition of equable progress. In like manner the *equation of the center* of the sun or



of the moon is the angle which must be added to, or subtracted from, the actual advance of the luminary in the heavens, in order to make its motion equable. Besides the equation of the center of the moon, which represents the first and greatest of her deviations from equable motion, there are many other *equations*, by the application of which her motion is brought nearer and nearer to perfect uniformity. The second of these equations is called the *evection*, the third the *variation*, the fourth the *annual equation*. The motion of the sun as affected by its inequalities is called his *anomaly*, which term denotes inequality. In the History of Astronomy, we find that the inequable motions of the sun, moon, and planets were, in a great measure, reduced to rule and system by the Greeks, by the aid of an hypothesis of circles, revolving, and carrying in their motion other circles which also revolved. This hypothesis introduced many technical terms, as *deferent*, *epicycle*, *eccentric*. In like manner, the theories which have more recently taken the place of the theory of epicycles have introduced other technical terms, as the *elliptical orbit*, the *radius vector*, and the *equable description of areas* by this radius, which phrases express the true laws of the planetary motions.

There is no subject on which theoretical views have been so long and so extensively prevalent as astronomy, and therefore no other science in which there are so many technical terms of the kind we are now considering. But in other subjects also, so far as theories have been established, they have been accompanied by the introduction or fixation of technical terms. Thus, as we have seen in the examination of the foundations of mechanics, the terms *force* and *inertia* derive their precise meaning from a recognition of the first law of motion; *accelerating force* and *composition of motion* involve the second

law; *moving force*, *momentum*, *action* and *reaction*, are expressions which imply the third law. The term *vis viva* was introduced to express a general property of moving bodies; and other terms have been introduced for like purposes, as *impetus* by Smeaton, and *work done*, by other engineers. In the recent writings of several French engineers, the term *travail* is much employed, to express the work done and the force which does it: this term has been rendered by *labouring force*. The proposition which was termed the *hydrostatic paradox* had this name in reference to its violating a supposed law of the action of forces. The verb to *gravitate*, and the abstract term *gravitation*, sealed the establishment of Newton's theory of the solar system.

In some of the sciences, opinions, either false, or disguised in very fantastical imagery, have prevailed; and the terms which have been introduced during the reign of such opinions, bear the impress of the time. Thus in the days of alchemy, the substances with which the operator dealt were personified; and a metal when exhibited pure and free from all admixture was considered as a little king, and was hence called a *regulus*, a term not yet quite obsolete. In like manner, a substance from which nothing more of any value could be extracted, was dead, and was called a *caput mortuum*. Quick silver, that is, live silver (*argentum vivum*), was killed by certain admixtures, and was *revived* when restored to its pure state.

We find a great number of medical terms which bear the mark of opinions formerly prevalent among physicians; and though these opinions hardly form a part of the progress of science, and were not presented in our History, we may notice some of these terms as examples of the mode in which words involve in their derivation obsolete opinions. Such words as *hysterics*, *hypochondriac*, *melancholy*, *cholera*, *colic*, *quinsey* (*squinantia*,

συνάγχη, a suffocation), *megrin*, *migraine* (*hemicranium*, the middle of the skull), *rickets*, (*rachitis*, from *ῥαχίς*, the backbone), *palsy*, (*paralysis*, παραλύσις,) *apoplexy* (ἀποπληξία, a stroke), *emroids*, (αἱμορροΐδες, *hemorrhoids*, a flux of blood), *imposthume*, (corrupted from *aposteme*, ἀπόστημα, an abscess), *phthisic* (φθίσις, consumption), *tympany* (τυμπαλία, swelling), *dropsy* (*hydropsy*, ὕδρωψ,) *sciatica*, isciatica (ἰσχιαδική, from ἰσχίον, the hip), *catarrh* (κατάρρους, a flowing down), *diarrhœa* (διαρροία, a flowing through), *diabetes* (διαβήτης, a passing through), *dysentery* (δυσεντερία, a disorder of the entrails), *arthritic pains* (from ἄρθρα, the joints), are names derived from the supposed or real seat and circumstances of the diseases. The word from which the first of the above names is derived (ὑστέρα, the last place,) signifies the womb, according to its order in a certain systematic enumeration of parts. The second word, *hypochondriac*, means something affecting the viscera below the cartilage of the breastbone, which cartilage is called χόνδρος; *melancholy* and *cholera* derive their names from supposed affections of χολή, the bile. *Colic* is that which affects the *colon* (κώλον), the largest member of the bowels. A disorder of the eye is called *gutta serena* (the "drop serene" of Milton), in contradistinction to *gutta turbida*, in which the impediment to vision is perceptibly opaque. Other terms also record the opinions of the ancient anatomists, as *duodenum*, a certain portion of the intestines, which they estimated as twelve inches long. We might add other allusions, as the *tendon of Achilles*.

Astrology also supplied a number of words founded upon fanciful opinions; but this study having been expelled from the list of sciences, such words now survive only so far as they have found a place in common language. Thus men were termed *mercurial*, *martial*, *jovial*, or *saturnine*, accordingly as their characters were

supposed to be determined by the influence of the planets, Mercury, Mars, Jupiter, or Saturn. Other expressions, such as *disastrous*, *ill-starred*, *exorbitant*, *lord of the ascendant*, and hence *ascendancy*, *influence*, a *sphere of action*, and the like, may serve to show how extensively astrological opinions have affected language, though the doctrine is no longer a recognized science.

The preceding examples will make it manifest that opinions, even of a recondite and complex kind, are often implied in the derivation of words; and thus will show how scientific terms, framed by the cultivators of science, may involve received hypotheses and theories. When terms are thus constructed, they serve not only to convey with ease, but to preserve steadily and to diffuse widely, the opinions which they thus assume. Moreover, they enable the speculator to employ these complex conceptions, the creations of science, and the results of much labour and thought, as readily and familiarly as if they were convictions borrowed at once from the senses. They are thus powerful instruments in enabling philosophers to ascend from one step of induction and generalization to another; and hereby contribute powerfully to the advance of knowledge and truth.

It should be noticed, before we proceed, that the names of natural objects, when they come to be considered as the objects of a science, are selected according to the processes already enumerated. For the most part, the natural historian adopts the common names of animals, plants, mineral, gems, and the like, and only endeavours to secure their steady and consistent application. But many of these names imply some peculiar, often fanciful, belief respecting the object.

Various plants derive their names from their supposed virtues, as *herniaria*, *rupture-wort*; or from legends, as *herba Sancti Johannis*, *St. John's wort*. The same is the case with minerals: thus the *topaz*

was asserted to come from an island so shrouded in mists that navigators could only *conjecture* (τοπαῖζειν) where it was. In these latter cases, however, the legend is often not the true origin of the name, but is suggested by it.

The privilege of constructing names where they are wanted, belongs to natural historians no less than to the cultivators of physical science; yet in the ancient world, writers of the former class appear rarely to have exercised this privilege, even when they felt the imperfections of the current language. Thus Aristotle repeatedly mentions classes of animals which have no name, as co-ordinate with classes that have names; but he hardly ventures to propose names which may supply these defects\*. The vast importance of nomenclature in natural history was not recognized till the modern period.

We have, however, hitherto considered only the formation or appropriation of single terms in science; except so far as several terms may in some instances be connected by reference to a common theory. But when the value of technical terms began to be fully appreciated, philosophers proceeded to introduce them into their sciences more copiously and in a more systematic manner. In this way, the modern history of technical language has some features of a different aspect from the ancient; and must give rise to a separate Aphorism.

## APHORISM II.

*In the Modern Period of Science, besides the three processes anciently employed in the formation of technical terms, there have been introduced Systematic Nomen-*

\* In his *History of Animals*, (Book i. chap. vi.), he says, that the great classes of animals are Quadrupeds, Birds, Fishes, Whales (*Cetaceans*), Oysters (*Testaceans*), animals like crabs which have no general name (*Crustaceans*), soft animals (*Mollusks* and *Insects*). He does, however, call the Crustaceans by a name (*Malacostraca*, soft-shelled) which has since been adopted by Naturalists.

*clature, Systematic Terminology, and the Systematic Modification of Terms to express theoretical relations\*.*

WRITERS upon science have gone on up to modern times forming such technical terms as they had occasion for, by the three processes above described;—namely, appropriating and limiting words in common use;—constructing for themselves words descriptive of the conception which they wished to convey;—or framing terms which by their signification imply the adoption of a theory. Thus among the terms introduced by the study of the connexion between magnetism and electricity, the word *pole* is an example of the first kind; the name of the subject, *electro-magnetism*, of the second; and the term *current*, involving an hypothesis of the motion of a fluid, is an instance of the third class. In chemistry, the term *salt* was adopted from common language, and its meaning extended to denote any compound of a certain kind; the term *neutral salt* implied the notion of a balanced opposition in the two elements of the compound; and such words as *subacid* and *superacid*, invented on purpose, were introduced to indicate the cases in which this balance was not attained. Again, when the phlogistic theory of chemistry was established, the term *phlogiston* was introduced to express the theory, and from this such terms as *phlogisticated* and *dephlogisticated* were derived, exclusively words of science. But in such instances as have just been given, we approach towards a systematic modification of terms, which is a peculiar process of modern times. Of this, modern chemistry forms a prominent example, which we shall soon consider, but we shall first notice the other processes mentioned in the Aphorism.

\* On the subject of Terminology and Nomenclature, see also Aphorisms LXXXVIII. and XCVIII. concerning Ideas, and Book VIII. chap. ii. of the *Philosophy*.

I. In ancient times, no attempt was made to invent or select a Nomenclature of the objects of Natural History which should be precise and permanent. The omission of this step by the ancient naturalists gave rise to enormous difficulty and loss of time when the sciences resumed their activity. We have seen in the history of the sciences of classification, and of botany in especial\*, that the early cultivators of that study in modern times endeavoured to identify all the plants described by Greek and Roman writers with those which grow in the north of Europe; and were involved in endless confusion†, by the multiplication of names of plants, at the same time superfluous and ambiguous. The *Synonymies* which botanists (Bauhin and others) found it necessary to publish, were the evidences of these inconveniences. In consequence of the defectiveness of the ancient botanical nomenclature, we are even yet uncertain with respect to the identification of some of the most common trees mentioned by classical writers‡. The ignorance of botanists respecting the importance of nomenclature operated in another manner to impede the progress of science. As a good nomenclature presupposes a good system of classification, so, on the other hand, a system of classification cannot become permanent without a corresponding nomenclature. Cæsalpinus, in the sixteenth century§, published an excellent system of arrangement for plants; but this, not being connected with any system of names, was never extensively accepted, and soon fell into oblivion. The business of framing a scientific botanical classification was in this way delayed for about a century. In the same manner, Willoughby's classification of fishes,

\* *Hist. Ind. Sci.*, B. XVI. c. ii.

† For instance, whether the *fagus* of the Latins be the beech or the chestnut.

‡ *Hist. Ind. Sci.*, B. XVI. c. iii. sect. 3.

§ *Ibid.*, B. XVI. c. iii. sect. 2.

though, as Cuvier says, far better than any which preceded it, was never extensively adopted, in consequence of having no nomenclature connected with it.

II. Probably one main cause which so long retarded the work of fixing at the same time the arrangement and the names of plants, was the great number of minute and diversified particulars in the structure of each plant which such a process implied. The stalks, leaves, flowers, and fruits of vegetables, with their appendages, may vary in so many ways, that common language is quite insufficient to express clearly and precisely their resemblances and differences. Hence botany required not only a fixed system of *names* of plants, but also an artificial system of phrases fitted to *describe* their parts: not only a *Nomenclature*, but also a *Terminology*. The Terminology was, in fact, an instrument indispensably requisite in giving fixity to the Nomenclature. The recognition of the kinds of plants must depend upon the exact comparison of their resemblances and differences; and to become a part of permanent science, this comparison must be recorded in words.

The formation of an exact descriptive language for botany was thus the first step in that systematic construction of the technical language of science, which is one of the main features in the intellectual history of modern times. The ancient botanists, as Decandolle\* says, did not make any attempt to select terms of which the sense was rigorously determined; and each of them employed in his descriptions the words, metaphors, or periphrases which his own genius suggested. In the History of Botany†, I have noticed some of the persons who contributed to this improvement. "Clusius," it is there stated, "first taught botanists to describe well.

\* *Theor. Elem. de Bot.*, p. 327.

† *Hist. Ind. Sci.*, B xvi. c. iii. sect. 3.



He introduced exactitude, precision, neatness, elegance, method: he says nothing superfluous; he omits nothing necessary." This task was further carried on by Jung and Ray\*. In these authors we see the importance which began to be attached to the exact definition of descriptive terms; for example, Ray quotes Jung's definition of *Caulis*, a stalk.

The improvement of descriptive language, and the formation of schemes of classification of plants, went on gradually for some time, and was much advanced by Tournefort. But at last Linnæus embodied and followed out the convictions which had gradually been accumulating in the breasts of botanists; and by remodelling throughout both the terminology and the nomenclature of botany, produced one of the greatest reforms which ever took place in any science. He thus supplied a conspicuous example of such a reform, and a most admirable model of a language, from which other sciences may gather great instruction. I shall not here give any account of the terms and words introduced by Linnæus. They have been exemplified in the *History of Science*†; and the principles which they involve I shall consider separately hereafter. I will only remind the reader that the great simplification in *nomenclature* which was the result of his labours, consisted in designating each kind of plant by a *binary* term consisting of the name of the *genus* combined with that of the *species*: an artifice seemingly obvious, but more convenient in its results than could possibly have been anticipated.

Since Linnæus, the progress of Botanical Anatomy and of Descriptive Botany have led to the rejection of several inexact expressions, and to the adoption of several new terms, especially in describing the structure of the

\* *Hist. Ind. Sci.*, B. XVI. c. iii. sect. 3. (about A. D. 1660).

† *Ib.*, c. iv. sect. 1—3.

fruit and the parts of cryptogamous plants. Hedwig, Medikus, Necker, Desvaux, Mirbel, and especially Gærtner, Link, and Richard, have proposed several useful innovations, in these as in other parts of the subject; but the general mass of the words now current consists still, and will probably continue to consist, of the terms established by the Swedish Botanist\*.

When it was seen that botany derived so great advantages from a systematic improvement of its language, it was natural that other sciences, and especially classificatory sciences, should endeavour to follow its example. This attempt was made in Mineralogy by Werner, and afterwards further pursued by Mohs. Werner's innovations in the descriptive language of Mineralogy were the result of great acuteness, an intimate acquaintance with minerals, and a most methodical spirit: and were in most respects great improvements upon previous practices. Yet the introduction of them into Mineralogy was far from regenerating that science, as Botany had been regenerated by the Linnæan reform. It would seem that the perpetual scrupulous attention to most minute differences, (as of lustre, colour, fracture,) the greater part of which are not really important, fetters the mind, rather than disciplines it or arms it for generalization. Cuvier has remarked† that Werner, after his first *Essay on the Characters of Minerals*, wrote little; as if he had been afraid of using the system which he had created, and desirous of escaping from the chains which he had imposed upon others. And he justly adds, that Werner dwelt least, in his descriptions, upon that which is really the most important feature of all, the crystalline structure. This, which is truly a definite character, like those of Botany, does, when it can be clearly discerned, determine the place of the mineral in a system. This, there-

\* De Candolle, *Th. Elem.*, p 307.

† *Eloges*, II. 314.

fore, is the character which, of all others, ought to be most carefully expressed by an appropriate language. This task, hardly begun by Werner, has since been fully executed by others, especially by Romé de l'Isle, Haüy, and Mohs. All the forms of crystals can be described in the most precise manner by the aid of the labours of these writers and their successors. But there is one circumstance well worthy our notice in these descriptions. It is found that the language in which they can best be conveyed is not that of words, but of *symbols*. The relations of space which are involved in the forms of crystalline bodies, though perfectly definite, are so complex and numerous, that they cannot be expressed, except in the language of mathematics: and thus we have an extensive and recondite branch of mathematical science, which is, in fact, only a part of the Terminology of the mineralogist.

The Terminology of Mineralogy being thus reformed, an attempt was made to improve its Nomenclature also, by following the example of Botany. Professor Mohs was the proposer of this innovation. The names framed by him were, however, not composed of two but of three elements, designating respectively the Species, the Genus, and the Order\*: thus he has such species as *Rhomboidal Lime Haloide*, *Octahedral Fluor Haloide*, *Prismatic Hal Baryte*. These names have not been generally adopted; nor is it likely that any names constructed on such a scheme will find acceptance among mineralogists, till the higher divisions of the system are found to have some definite character. We see no real mineralogical significance in Mohs's Genera and Orders, and hence we do not expect them to retain a permanent place in the science.

The only systematic names which have hitherto been

\* *Hist. Ind. Sci.*, B. xv. c. ix.

generally admitted in Mineralogy, are those expressing the chemical constitution of the substance; and these belong to a system of technical terms different from any we have yet spoken of, namely to terms formed by systematic modification.

III. The language of Chemistry was already, as we have seen, tending to assume a systematic character, even under the reign of the phlogiston theory. But when oxygen succeeded to the throne, it very fortunately happened that its supporters had the courage and the foresight to undertake a completely new and systematic recoinage of the terms belonging to the science. The new nomenclature was constructed upon a principle hitherto hardly applied in science, but eminently commodious and fertile; namely, the principle of indicating a modification of relations of elements, by a change in the termination of the word. Thus the new chemical school spoke of *sulphuric* and *sulphurous* acids; of *sulphates* and *sulphites* of bases; and of *sulphurets* of metals; and in like manner, of *phosphoric* and *phosphorous* acids, of *phosphates*, *phosphites*, *phosphurets*. In this manner a nomenclature was produced, in which the very name of a substance indicated at once its constitution and place in the system.

The introduction of this chemical language can never cease to be considered one of the most important steps ever made in the improvement of technical terms; and as a signal instance of the advantages which may result from artifices apparently trivial, if employed in a manner conformable to the laws of phenomena, and systematically pursued. It was, however, proved that this language, with all its merits, had some defects. The relations of elements in composition were discovered to be more numerous than the modes of expression which the terminations supplied. Besides the *sulphurous* and *sul-*

phuric acids, it appeared there were others; these were called the *hyposulphurous* and *hyposulphuric*: but these names, though convenient, no longer implied, by their form, any definite relation. The compounds of Nitrogen and Oxygen are, in order, the *Protoxide*, the *Deutoxide* or *Binoxide*; *Hyponitrous Acid*, *Nitrous Acid*, and *Nitric Acid*. The nomenclature here ceases to be systematic. We have three oxides of Iron, of which we may call the first the *Protoxide*, but we cannot call the others the *Deutoxide* and *Tritoxide*, for by doing so we should convey a perfectly erroneous notion of the proportions of the elements. They are called the *Protoxide*, the *Black Oxide*, and the *Peroxide*. We are here thrown back upon terms quite unconnected with the system.

Other defects in the nomenclature arose from errors in the theory; as for example the names of the muriatic, oxymuriatic, and hyperoxymuriatic acids; which, after the establishment of the new theory of chlorine, were changed to *hydrochloric acid*, *chlorine*, and *chloric acid*.

Thus the chemical system of nomenclature, founded upon the oxygen theory, while it shows how much may be effected by a good and consistent scheme of terms, framed according to the real relations of objects, proves also that such a scheme can hardly be permanent in its original form, but will almost inevitably become imperfect and anomalous, in consequence of the accumulation of new facts, and the introduction of new generalizations. Still, we may venture to say that such a scheme does not, on this account, become worthless; for it not only answers its purpose in the stage of scientific progress to which it belongs:—so far as it is not erroneous, or merely conventional, but really systematic and significant of truth, its terms can be translated at once into the language of any higher generalization which is after-

wards arrived at. If terms express relations really ascertained to be true, they can never lose their value by any change of the received theory. They are like coins of pure metal, which, even when carried into a country which does not recognize the sovereign whose impress they bear, are still gladly received, and may, by the addition of an explanatory mark, continue part of the common currency of the country.

These two great instances of the reform of scientific language, in Botany and in Chemistry, are much the most important and instructive events of this kind which the history of science offers. It is not necessary to pursue our historical survey further. Our remaining Aphorisms respecting the Language of Science will be collected and illustrated indiscriminately, from the precepts and the examples of preceding philosophers of all periods.

We may, however, remark that Aphorisms III., IV., V., VI., VII., respect peculiarly the Formation of Technical Terms by the Appropriation of Common Words, while the remaining ones apply to the Formation of New Terms.

It does not appear possible to lay down a system of rules which may determine and regulate the construction of all technical terms, on all the occasions on which the progress of science makes them necessary or convenient. But if we can collect a few maxims such as have already offered themselves to the minds of philosophers, or such as may be justified by the instances by which we shall illustrate them, these maxims may avail to guide us in doubtful cases, and to prevent our aiming at advantages which are unattainable, or being disturbed by seeming imperfections which are really no evils. I shall therefore state such maxims of this kind as seem most sound and useful.

## APHORISM III.

*In framing scientific terms, the appropriation of old words is preferable to the invention of new ones.*

THIS maxim is stated by Bacon in his usual striking manner. After mentioning *Metaphysic*, as one of the divisions of Natural Philosophy, he adds\*: "Wherein I desire it may be conceived that I use the word *metaphysic* in a differing sense from that that is received: and in like manner I doubt not but it will easily appear to men of judgment that in this and other particulars, wheresoever my conception and notion may differ from the ancient, yet I am studious to keep the ancient terms. For, hoping well to deliver myself from mistaking by the order and perspicuous expressing of that I do propound; I am otherwise zealous and affectionate to recede as little from antiquity, either in terms or opinions, as may stand with truth, and the proficience of knowledge, . . . To me, that do desire, as much as lieth in my pen, to ground a sociable intercourse between antiquity and proficience, it seemeth best to keep a way with antiquity *usque ad aras*; and therefore to retain the ancient terms, though I sometimes alter the uses and definitions; according to the moderate proceeding in civil governments, when, although there be some alteration, yet that holdeth which Tacitus wisely noteth, *eadem magistratuum vocabula*."

We have had before us a sufficient number of examples of scientific terms thus framed; for they formed the first of three classes which we described in the First Aphorism. And we may again remark, that science, when she thus adopts terms which are in common use, always limits and fixes their meaning in a technical manner. We may also repeat here the warning already given respecting terms of this kind, that they are peculi-

\* *De Augm.*, Lib. III. c. iv.

arly liable to mislead readers who do not take care to understand them in their technical instead of their common signification. *Force, momentum, inertia, impetus, vis viva*, are terms which are very useful, if we rigorously bear in mind the import which belongs to each of them in the best treatises on Mechanics; but if the reader content himself with conjecturing their meaning from the context, his knowledge will be confused and worthless.

In the application of this Third Aphorism, other rules are to be attended to, which I add.

#### APHORISM IV.

*When common words are appropriated as technical terms, their meaning and relations in common use should be retained as far as can conveniently be done.*

I WILL state an example in which this rule seems to be applicable. Mr. Davies Gilbert\* has recently proposed the term *efficiency* to designate the work which a machine, according to the force exerted upon it, is capable of doing; the work being measured by the weight raised, and the space through which it is raised, jointly. The usual term employed among engineers for the work which a machine actually does, measured in the way just stated, is *duty*. But as there appears to be a little incongruity in calling that work *efficiency* which the machine *ought* to do, when we call that work *duty* which it really does, I have proposed to term these two quantities *theoretical efficiency* and *practical efficiency*, or *theoretical duty* and *practical duty*†.

Since common words are often vague in their mean-

\* *Phil. Trans.* 1827, p. 25.

† The term *travail* is used by French engineers, to express *efficiency* or *theoretical duty*. This term has been rendered in English by *labouring force*.



ing, I add as a necessary accompaniment to the Third Aphorism the following:—

#### APHORISM V.

*When common words are appropriated as technical terms, their meaning may be modified, and must be rigorously fixed.*

THIS is stated by Bacon in the above extract: “to retain the ancient terms, though I sometimes *alter the uses and definitions*.” The scientific use of the term is in all cases much more precise than the common use. The loose notions of *velocity* and *force* for instance, which are sufficient for the usual purposes of language, require to be fixed by exact measures when these are made terms in the science of Mechanics.

This scientific fixation of the meaning of words is to be looked upon as a matter of convention, although it is in reality often an inevitable result of the progress of science. *Momentum* is conventionally defined to be the product of the numbers expressing the weight and the velocity; but then, it could be of no use in expressing the laws of motion if it were defined otherwise.

Hence it is no valid objection to a scientific term that the word in common language does not mean exactly the same as in its common use. It is no sufficient reason against the use of the term *acid* for a class of bodies, that all the substances belonging to this class are not sour. We have seen that a *trapezium* is used in geometry for any four-sided figure, though originally it meant a figure with two opposite sides parallel and the two others equal. A certain stratum which lies below the chalk is termed by English geologists *the green sand*. It has sometimes been objected to this denomination that the stratum has very frequently no tinge of green, and that it is often composed of lime with little or no

sand. Yet the term is a good technical term in spite of these apparent improprieties; so long as it is carefully applied to that stratum which is geologically equivalent to the greenish sandy bed to which the appellation was originally applied.

When it appeared that *geometry* would have to be employed as much at least about the heavens as the earth, Plato exclaimed against the folly of calling the science by such a name; since the word signifies "earth-measuring;" yet the word *geometry* has retained its place and answered its purpose perfectly well up to the present day.

But though the meaning of the term may be modified or extended, it must be rigorously fixed when it is appropriated to science. This process is most abundantly exemplified by the terminology of Natural History, and especially of Botany, in which each term has a most precise meaning assigned to it. Thus Linnæus established exact distinctions between *fasciculus*, *capitulum*, *racemus*, *thyrsus*, *paniculus*, *spica*, *amentum*, *corymbus*, *umbella*, *cyma*, *verticillus*; or, in the language of English Botanists, *a tuft*, *a head*, *a cluster*, *a bunch*, *a panicle*, *a spike*, *a catkin*, *a corymb*, *an umbel*, *a cyme*, *a whorl*. And it has since been laid down as a rule\*, that each organ ought to have a separate and appropriate name; so that the term *leaf*, for instance, shall never be applied to *a leaflet*, *a bractea*, or *a sepal* of the calyx.

Botanists have not been content with fixing the meaning of their terms by verbal definition, but have also illustrated them by figures, which address the eye. Of these, as excellent modern examples, may be mentioned those which occur in the works of Mirbel†, and Lindley‡.

\* De Candolle, *Theor. El.*, 328.

† *Elémens de Botanique*.

‡ *Elements of Botany*.

## APHORISM VI.

*When common words are appropriated as technical terms, this must be done so that they are not ambiguous in their application.*

AN example will explain this maxim. The conditions of a body, as a solid, a liquid, and an air, have been distinguished as different *forms* of the body. But the word *form*, as applied to bodies, has other meanings ; so that if we were to inquire in *what form* water exists in a snow-cloud, it might be doubted whether the forms of crystallization were meant, or the different forms of ice, water, and vapour. Hence I have proposed\* to reject the term *form* in such cases, and to speak of the different *consistence* of a body in these conditions. The term *consistence* is usually applied to conditions between solid and fluid ; and may without effort be extended to those limiting conditions. And though it may appear more harsh to extend the term *consistence* to the state of air, it may be justified by what has been said in speaking of Aphorism V.

I may notice another example of the necessity of avoiding ambiguous words. A philosopher who makes method his study, would naturally be termed a *methodist* ; but unluckily this word is already appropriated to a religious sect : and hence we could hardly venture to speak of Cæsalpinus, Ray, Morison, Rivinus, Tournefort, Linnæus, and their successors, as *botanical methodists*. Again, by this maxim, we are almost debarred from using the term *physician* for a cultivator of the science of physics, because it already signifies a practiser of physic. We might, perhaps, still use *physician* as the equivalent of the French *physicien*, in virtue of Aphorism V. ; but probably it would be better to form a new word. Thus

\* *Hist. Ind. Sci.*, B. x. c. ii. sect. 2.

we may say, that while the Naturalist employs principally the ideas of resemblance and life, the *Physicist* proceeds upon the ideas of force, matter, and the properties of matter.

Whatever may be thought of this proposal, the maxim which it implies is frequently useful. It is this.

#### APHORISM VII.

*It is better to form new words as technical terms, than to employ old ones in which the last three Aphorisms cannot be complied with.*

THE principal inconvenience attending the employment of new words constructed expressly for the use of science, is the difficulty of effectually introducing them. Readers will not readily take the trouble to learn the meaning of a word, in which the memory is not assisted by some obvious suggestion connected with the common use of language. When this difficulty is overcome, the new word is better than one merely appropriated; since it is more secure from vagueness and confusion. And in cases where the inconveniences belonging to a scientific use of common words become great and inevitable, a new word must be framed and introduced.

The Maxims which belong to the construction of such words will be stated hereafter; but I may notice an instance or two tending to show the necessity of the Maxim now before us.

The word *Force* has been appropriated in the science of Mechanics in two senses: as indicating the cause of motion; and again, as expressing certain measures of the effects of this cause, in the phrases *accelerating force* and *moving force*. Hence we might have occasion to speak of the accelerating or moving force of a certain *force*; for instance, if we were to say that the force which governs the motions of the planets resides

in the sun; and that the accelerating force *of* this *force* varies only with the distance, but its moving force varies as the product of the mass of the sun and the planet. This is a harsh and incongruous mode of expression; and might have been avoided, if, instead of *accelerating force* and *moving force*, single abstract terms had been introduced by Newton: if, for instance, he had said that the velocity generated in a second measures the *accelerativity* of the force which produces it, and the momentum produced in a second measures the *motivity* of the force.

The science which treats of heat has hitherto had no special designation: treatises upon it have generally been termed treatises *On Heat*. But this practice of employing the same term to denote the property and the science which treats of it, is awkward, and often ambiguous. And it is further attended with this inconvenience, that we have no adjective derived from the name of the science, as we have in other cases, when we speak of *acoustical* experiments and *optical* theories. This inconvenience has led various persons to suggest names for the Science of Heat. M. Comte terms it *Thermology*. In the *History of the Sciences*, I have named it *Thermotics*, which appears to me to agree better with the analogy of the names of other corresponding sciences, *Acoustics* and *Optics*.

*Electricity* is in the same condition as Heat; having only one word to express the property and the science. M. Le Comte proposes *Electrology*: for the same reason as before, I should conceive *Electrics* more agreeable to analogy. The coincidence of the word with the plural of Electric would not give rise to ambiguity; for *Electrics*, taken as the name of a science, would be singular, like *Optics* and *Mechanics*. But a term offers itself to express *common* or *machine* *Electrics*, which appears

worthy of admission, though involving a theoretical view. The received doctrine of the difference between Voltaic and Common Electricity is, that in the former case the fluid must be considered as in motion, in the latter as at rest. The science which treats of the former class of subjects is commonly termed *Electrodynamics*, which obviously suggests the name *Electrostatics* for the latter.

The subject of the Tides is, in like manner, destitute of any name which designates the science concerned about it. I have ventured to employ the term *Tidology*, having been much engaged in tidological researches.

Many persons possess a peculiarity of vision, which disables them from distinguishing certain colours. On examining many such cases, we find that in all such persons the peculiarities are the same; all of them confounding scarlet with green, and pink with blue. Hence they form a class, which, for the convenience of physiologists and others, ought to have a fixed designation. Instead of calling them, as has usually been done, "persons having a peculiarity of vision," we might take a Greek term implying this meaning, and term them *Idiopts*.

But my business at present is not to speak of the selection of new terms when they are introduced, but to illustrate the maxim that the necessity for their introduction often arises. The construction of new terms will be treated of subsequently.

#### APHORISM VIII.

*Terms must be constructed and appropriated so as to be fitted to enunciate simply and clearly true general propositions.*

THIS Aphorism may be considered as the fundamental principle and supreme rule of all scientific terminology. It is asserted by Cuvier, speaking of a particular case.

Thus he says\* of Gmelin, that by placing the lamantin in the genus of morses, and the siren in the genus of eels, he had rendered every general proposition respecting the organization of those genera impossible.

The maxim is true of words appropriated as well as invented, and applies equally to the mathematical, chemical, and classificatory sciences. With regard to most of these, and especially the two former classes, it has been abundantly exemplified already, in what has previously been said, and in the *History of the Sciences*. For we have there had to notice many technical terms, with the occasions of their introduction; and all these occasions have involved the intention of expressing in a convenient manner some truth or supposed truth. The terms of Astronomy were adopted for the purpose of stating and reasoning upon the relations of the celestial motions, according to the doctrine of the sphere, and the other laws which were discovered by astronomers. The few technical terms which belong to Mechanics, *force*, *velocity*, *momentum*, *inertia*, &c., were employed from the first with a view to the expression of the laws of motion and of rest; and were, in the end, limited so as truly and simply to express those laws when they were fully ascertained. In Chemistry, the term *phlogiston* was useful, as has been shown in the *History*, in classing together processes which really are of the same nature; and the nomenclature of the *oxygen* theory was still preferable, because it enabled the chemist to express a still greater number of general truths.

To the connexion here asserted, of theory and nomenclature, we have the testimony of the author of the oxygen theory. In the Preface to his *Chemistry*, Lavoisier says:—"Thus while I thought myself employed only in forming a Nomenclature, and while I proposed to

\* *Règne Animal*, Introd. viii.

myself nothing more than to improve the chemical language, my work transformed itself by degrees, without my being able to prevent it, into a Treatise on the Elements of Chemistry." And he then proceeds to show how this happened.

It is, however, mainly through the progress of Natural History in modern times, that philosophers have been led to see the importance and necessity of new terms in expressing new truths. Thus Harvey, in the Preface to his work on Generation, says:—"Be not offended if in setting out the History of the Egg I make use of a new method, and sometimes of unusual terms. For as they which find out a new plantation and new shores call them by names of their own coining, which posterity afterwards accepts and receives, so those that find out new secrets have good title to their compellation. And here, methinks, I hear Galen advising: If we consent in the things, contend not about the words."

The Nomenclature which answers the purposes of Natural History is a Systematic Nomenclature, and will be further considered under the next Aphorism. But we may remark, that the Aphorism now before us governs the use of words, not in science only, but in common language also. Are we to apply the name *fish* to animals of the whale kind? The answer is determined by our present rule: we are to do so, or not, accordingly as we can best express true propositions. If we are speaking of the internal structure and physiology of the animal, we must not call them *fish*; for in these respects they deviate widely from fishes: they have warm blood, and produce and suckle their young as land quadrupeds do. But this would not prevent our speaking of the *whale-fishery*, and calling such animals *fish* on all occasions connected with this employment; for the relations thus arising depend upon the animal's living in the



water, and being caught in a manner similar to other fishes. A plea that human laws which mention fish do not apply to whales, would be rejected at once by an intelligent judge.

#### APHORISM IX.

*In the Classificatory Sciences, a Systematic Nomenclature is necessary; and the System and the Nomenclature are each essential to the utility of the other.*

THE inconveniences arising from the want of a good Nomenclature were long felt in Botany, and are still felt in Mineralogy. The attempts to remedy them by *Synonymies* are very ineffective, for such comparisons of synonymes do not supply a systematic nomenclature; and such a one alone can enable us to state general truths respecting the objects of which the classificatory sciences treat. The *System* and the *Names* ought to be introduced together; for the former is a collection of asserted analogies and resemblances, for which the latter provide simple and permanent expressions. Hence it has repeatedly occurred in the progress of Natural History, that good Systems did not take root, or produce any lasting effect among naturalists, because they were not accompanied by a corresponding Nomenclature. In this way, as we have already noticed, the excellent botanical System of Cæsalpinus was without immediate effect upon the science. The work of Willoughby, as Cuvier says\*, forms an epoch, and a happy epoch in Ichthyology; yet because Willoughby had no Nomenclature of his own, and no fixed names for his genera, his immediate influence was not great. Again, in speaking of Schlotheim's work containing representations of fossil vegetables, M. Adolphe Brongniart observes† that the figures and descriptions are so good, that if the author

\* *Hist. des Poissons*, Pref.

† *Prodrom. Veg. Foss.*, p. 3.

had established a nomenclature for the objects he describes, his work would have become the basis of all succeeding labours on the subject.

As additional examples of cases in which the improvement of classification, in recent times, has led philosophers to propose new names, I may mention the term *Pæcilite*, proposed by Mr. Conybeare to designate the group of strata which lies below the oolites and lias, including the new red or variegated sandstone, with the keuper above, and the magnesian limestone below it. Again, the transition districts of our island have recently been reduced to system by Professor Sedgwick and Mr. Murchison; and this step has been marked by the terms *Cambrian* system, and *Silurian* system, applied to the two great groups of formations which they have respectively examined, and by several other names of the subordinate members of these formations.

Thus System and Nomenclature are each essential to the other. Without Nomenclature, the system is not permanently incorporated into the general body of knowledge, and made an instrument of future progress. Without System, the names cannot express general truths, and contain no reason why they should be employed in preference to any other names.

This has been generally acknowledged by the most philosophical naturalists of modern times. Thus Linæus begins that part of his *Botanical Philosophy* in which names are treated of, by stating that the foundation of botany is twofold, *Disposition* and *Denomination*; and he adds this Latin line,

Nomina si nescis perit et cognitio rerum.

And Cuvier, in the Preface to his *Animal Kingdom*, explains, in a very striking manner, how the attempt to connect zoology with anatomy led him, at the same time,

to reform the classifications, and to correct the nomenclature of preceding zoologists.

I have stated that in Mineralogy we are still destitute of a good nomenclature generally current. From what has now been said, it will be seen that it may be very far from easy to supply this defect, since we have, as yet, no generally received system of mineralogical classification. Till we know what are really different species of minerals, and in what larger groups these species can be arranged, so as to have common properties, we shall never obtain a permanent mineralogical nomenclature. Thus *Leucocyclite* and *Tesselite* are minerals previously confounded with *Apophyllite*, which Sir John Herschel and Sir David Brewster distinguished by those names, in consequence of certain optical properties which they exhibit. But are these properties definite distinctions? and are there any external differences corresponding to them? If not, can we consider them as separate species? and if not separate species, ought they to have separate names? In like manner, we might ask if *Augite* and *Hornblende* are really the same species, as Gustavus Rose has maintained? if *Diallage* and *Hypersthene* are not definitely distinguished, which has been asserted by Kobell? Till such questions are settled, we cannot have a fixed nomenclature in mineralogy. What appears the best course to follow in the present state of the science, I shall consider when we come to speak of the form of technical terms.

I may, however, notice here that the main Forms of systematic nomenclature are two:—terms which are produced by combining words of higher and lower generality, as the binary names, consisting of the name of the genus and the species, generally employed by natural historians since the time of Linnæus;—and terms in which some relation of things is indicated by a change

in the form of the word, for example, an alteration of its termination, of which kind of nomenclature we have a conspicuous example in the modern chemistry.

#### APHORISM X.

*New terms and changes of terms, which are not needed in order to express truth, are to be avoided.*

As the Seventh Aphorism asserted that novelties in language may be and ought to be introduced, when they aid the enunciation of truths, we now declare that they are not admissible in any other case. New terms and new systems of terms are not to be introduced, for example, in virtue of their own neatness or symmetry, or other merits, if there is no occasion for their use.

I may mention, as an old example of a superfluous attempt of this kind, an occurrence in the history of Astronomy. In 1628 John Bayer and Julius Schiller devised a *Cælum Christianum*, in which the common names of the planets, &c., were replaced by those of Adam, Moses, and the Patriarchs. The twelve Signs became the twelve Apostles, and the constellations became sacred places and things. Peireskius, who had to pronounce upon the value of this proposal, praised the piety of the inventors, but did not approve, he said\*, the design of perverting and confounding whatever of celestial information from the period of the earliest memory is found in books.

Nor are slight anomalies in the existing language of science sufficient ground for a change, if they do not seriously interfere with the expression of our knowledge. Thus Linnæus says† that a fair generic name is not to be exchanged for another though apter one: and‡ if we separate an old genus into several, we must try to find

\* Gassendi, *Vita Peireskii*, 300. † *Phil. Bot.*, 246. ‡ *Ib.*, 247.

names for them among the synonyms which describe the old genus. This maxim excludes the restoration of ancient names long disused, no less than the needless invention of new ones. Linnæus lays down this rule\*; and adds, that the botanists of the sixteenth century well nigh ruined botany by their anxiety to recover the ancient names of plants. In like manner Cuvier† laments it as a misfortune, that he has had to introduce many new names; and declares earnestly that he has taken great pains to preserve those of his predecessors.

The great bulk which the Synonymy of botany and of mineralogy have attained, shows us that this maxim has not been universally attended to. In these cases, however, the multiplication of different names for the same kind of object has arisen in general from ignorance of the identity of it under different circumstances, or from the want of a system which might assign to it its proper place. But there are other instances, in which the multiplication of names has arisen not from defect, but from excess, of the spirit of system. The love which speculative men bear towards symmetry and completeness is constantly at work, to make them create systems of classification more regular and more perfect than can be verified by the facts: and as good systems are closely connected with a good nomenclature, systems thus erroneous and superfluous lead to a nomenclature which is prejudicial to science. For although such a nomenclature is finally expelled, when it is found not to aid us in expressing the true laws of nature, it may obtain some temporary sway, during which, and even afterwards, it may be a source of much confusion.

We have a conspicuous example of such a result in the geological nomenclature of Werner and his school. Thus it was assumed, in Werner's system, that his *First*,

\* *Phil. Bot.*, 248.

† *Règne Anim.*, Pref. p. xvi.

*Second, and Third Flötz Limestone, his Old and New Red Sandstone*, were universal formations; and geologists looked upon it as their business to detect these strata in other countries. Names were thus assigned to the rocks of various parts of Europe, which created immense perplexity before they were again ejected. The geological terms which now prevail, for instance, those of Smith, are for the most part not systematic, but are borrowed from accidents, as localities, or popular names; as *Oxford Clay* and *Cornbrash*; and hence they are not liable to be thrust out on a change of system. On the other hand we do not find sufficient reason to accept the system of names of strata proposed by Mr. Conybeare in the *Introduction to the Geology of England and Wales*, according to which the *Carboniferous Rocks* are the *Medial Order*,—having above them the *Supermedial Order* (*New Red Sand, Oolites and Chalk*), and above these the *Superior Order* (*Tertiary Rocks*); and again,—having below, the *Submedial Order* (the *Transition Rocks*), and the *Inferior Order* (*Mica Slate, Gneiss, Granite*). For though these names have long been proposed, it does not appear that they are useful in enunciating geological truths. We may, it would seem, pronounce the same judgment respecting the system of geological names proposed by M. Alexander Brongniart, in his *Tableau des Terrains qui composent l'écorce du Globe*. He divides these strata into nine classes, which he terms *Terrains Alluviens, Lysiens, Pyrogenes, Clysmiens, Yzemiens, Hemilysiens, Agalysiens, Plutoniques, Vulcaniques*. These classes are again variously subdivided: thus the *Terrains Yzemiens* are *Thalassiques, Pelagiques, and Abyssiques*; and the *Abyssiques* are subdivided into *Lias, Keuper, Conchiliens, Pœciliens, Peneens, Rudimentaires, Entritiques, Houillers, Carbonifers* and *Gres Rouge Ancien*. Scarcely any amount

of new truths would induce geologists to burthen themselves at once with this enormous system of new names: but in fact, it is evident that any portion of truth, which any author can have brought to light, may be conveyed by means of a much simpler apparatus. Such a nomenclature carries its condemnation on its own face.

Nearly the same may be said of the systematic nomenclature proposed for mineralogy by Professor Mohs. Even if all his Genera be really natural groups, (a doctrine which we can have no confidence in till they are confirmed by the evidence of chemistry,) there is no necessity to make so great a change in the received names of minerals. His proceeding in this respect, so different from the temperance of Linnæus and Cuvier, has probably ensured a speedy oblivion to this part of his system. In crystallography, on the other hand, in which Mohs's improvements have been very valuable, there are several terms introduced by him, as *rhombohedron*, *scalenoedron*, *hemihedral*, *systems* of crystallization, which will probably be a permanent portion of the language of science.

I may remark, in general, that the only persons who succeed in making great alterations in the language of science, are not those who make names arbitrarily and as an exercise of ingenuity, but those who have much new knowledge to communicate; so that the vehicle is commended to general reception by the value of what it contains. It is only eminent discoverers to whom the authority is conceded of introducing a new system of names; just as it is only the highest authority in the state which has the power of putting a new coinage in circulation.

I will here quote some judicious remarks of Mr. Howard, which fall partly under this Aphorism, and partly under some which follow. He had proposed, as

names for the kinds of clouds, the following: *Cirrus*, *Cirrocumulus*, *Cirrostratus*, *Cumulostratus*, *Cumulus*, *Nimbus*, *Stratus*. In an abridgment of his views, given in the Supplement to the *Encyclopædia Britannica*, English names were proposed as the equivalents of these; *Curlcloud*, *Sondercloud*, *Wanecloud*, *Twaincloud*, *Stackencloud*, *Raincloud*, *Fallcloud*. Upon these Mr. Howard observes: "I mention these, in order to have the opportunity of saying that I do not adopt them. The names for the clouds which I deduced from the Latin, are but seven in number, and very easy to remember. They were intended as *arbitrary terms* for the *structure* of clouds, and the meaning of them was carefully fixed by a definition. The observer having once made himself master of this, was able to apply the term with correctness, after a little experience, to the subject under all its varieties of form, colour, or position. The new names, if meant to be another set of arbitrary terms, are superfluous; if intended to convey in themselves an explanation in English, they fail in this, by applying to some part or circumstance only of the definition; the *whole* of which must be kept in view to study the subject with success. To take for an example the first of the modifications. The term *cirrus* very readily takes an abstract meaning, equally applicable to the rectilinear as to the flexuous forms of the subject. But the name of *curl-cloud* will not, without some violence to its *obvious sense*, acquire this more extensive one: and will therefore be apt to mislead the reader rather than further his progress. Others of these names are as devoid of a meaning obvious to the English reader, as the Latin terms themselves. But the principal objection to English or any other local terms, remains to be stated. They take away from the nomenclature its general advantage of constituting, as far as it goes, an universal



language, by means of which the intelligent of every country may convey to each other their ideas without the necessity of translation."

I here adduce these as examples of the arguments against changing an established nomenclature. As grounds of selecting a new one, they may be taken into account hereafter.

#### APHORISM XI.

*Terms which imply theoretical views are admissible, as far as the theory is proved.*

It is not unfrequently stated that the circumstances from which the names employed in science borrow their meaning, ought to be facts and not theories. But such a recommendation implies a belief that facts are rigorously distinguished from theories and directly opposed to them; which belief, we have repeatedly seen, is unfounded. When theories are firmly established, they become facts; and names founded on such theoretical views are unexceptionable. If we speak of the *minor axis* of Jupiter's orbit, or of his *density*, or of *the angle of refraction*, or *the length of an undulation* of red light, we assume certain theories; but inasmuch as the theories are now the inevitable interpretation of ascertained facts, we can have no better terms to designate the conceptions thus referred to. And hence the rule which we must follow is, not that our terms must involve no theory, but that they imply the theory only in that sense in which it is the interpretation of the facts.

For example, the term *polarization* of light was objected to, as involving a theory. Perhaps the term was at first suggested by conceiving light to consist of particles having poles turned in a particular manner. But among intelligent speculators, the notion of polar-

ization soon reduced itself to the simple conception of opposite properties in opposite positions, which is a bare statement of the fact: and the term being understood to have this meaning, is a perfectly good term, and indeed the best which we can imagine for designating what is intended.

I need hardly add the caution, that names involving theoretical views not in accordance with facts are to be rejected. The following instances exemplify both the positive and the negative application of this maxim.

The distinction of *primary* and *secondary* rocks in geology was founded upon a theory; namely, that those which do not contain any organic remains were first deposited, and afterwards, those which contain plants and animals. But this theory was insecure from the first. The difficulty of making the separation which it implied, led to the introduction of a class of *transition*-rocks. And the recent researches of geologists lead them to the conclusion, that those rocks which are termed *primary*, may be the newest, not the oldest, productions of nature.

In order to avoid this incongruity, other terms have been proposed as substitutes for these. Mr. Lyell remarks\*, that granite, gneiss, and the like, form a class which should be designated by a common name; which name should not be of chronological import. He proposes *hypogene*, signifying "nether-formed;" and thus he adopts the theory that they have not assumed their present form and structure at the surface, but determines nothing of the period when they were produced.

These hypogene rocks, again, he divides into unstratified or *plutonic*, and altered, stratified, or *metamorphic*; the latter term implying the hypothesis that the stratified rocks to which it is applied have been altered, by the

\* *Princ. Geol.*, iv. 386.

effect of fire or otherwise, since they were deposited. That fossiliferous strata, in some cases at least, have undergone such a change, is demonstrable from facts\*.

The modern nomenclature of chemistry implies the oxygen theory of chemistry. Hence it has sometimes been objected to. Thus Davy, in speaking of the Lavoisierian nomenclature, makes the following remarks, which, however plausible they may sound, will be found to be utterly erroneous†. "Simplicity and precision ought to be the characteristics of a scientific nomenclature: words should signify *things*, or the *analogies* of things, and not *opinions*. . . . A substance in one age supposed to be simple, in another is proved to be compound, and *vice versâ*. A theoretical nomenclature is liable to continual alterations: *oxygenated muriatic acid* is as improper a term as *dephlogisticated marine acid*. Every school believes itself to be in the right: and if every school assumes to itself the liberty of altering the names of chemical substances in consequence of *new ideas* of their composition, there can be no permanency in the language of the science; it must always be confused and uncertain. Bodies which are *similar* to each other should always be classed together; and there is a presumption that their composition is *analogous*. *Metals, earths, alkalis*, are appropriate names for the bodies they represent, and independent of all speculation: whereas *oxides, sulphurets, and muriates* are terms founded upon opinions of the composition of bodies, some of which have been already found erroneous. The least dangerous mode of giving a systematic form to a language seems to be to signify the analogies of substances by some common sign affixed to the beginning or the termination of the word. Thus as the metals have been distinguished by a termination in *um*, as

\* *Elem. Geol.*, p. 17.

† *Elements of Chem. Phil.*, p. 46.

*aurum*, so their calciform or oxidated state might have been denoted by a termination in *a*, as *aura*: and no progress, however great, in the science could render it necessary that such a mode of appellation should be changed."

These remarks are founded upon distinctions which have no real existence. We cannot separate *things* from their *properties*, nor can we consider their properties and analogies in any other way than by having *opinions* about them. By contrasting *analogies* with *opinions*, it might appear as if the author maintained that there were certain analogies about which there was no room for erroneous opinions. Yet the analogies of chemical compounds, are, in fact, those points which have been most the subject of difference of opinion, and on which the revolutions of theories have most changed men's views. As an example of analogies which are still recognized under alterations of theory, the writer gives the relation of a metal to its oxide or calciform state. But this analogy of metallic oxides, as Red Copper or Iron Ore, to Calx, or burnt lime, is very far from being self-evident;—so far indeed, that the recognition of the analogy was a great step in chemical *theory*. The terms which he quotes, *oxygenated muriatic acid* (and the same may be said of *dephlogisticated marine acid*,) if improper, are so not because they involve theory, but because they involve false theory;—not because those who framed them did not endeavour to express analogies, but because they expressed analogies about which they were mistaken. Unconnected names, as *metals*, *earths*, *alkalis*, are good as the *basis* of a systematic nomenclature, but they are not substitutes for such a nomenclature. A systematic nomenclature is an instrument of great utility and power, as the modern history of chemistry has shown. It would be highly unphiloso-

phical to reject the use of such an instrument, because, in the course of the revolutions of science, we may have to modify, or even to remodel it altogether. Its utility is not by that means destroyed. It has retained, transmitted, and enabled us to reason upon, the doctrines of the earlier theory, so far as they are true; and when this theory is absorbed into a more comprehensive one, (for this, and not its refutation, is the end of a theory so far as it is true,) the nomenclature is easily translated into that which the new theory introduces. We have seen, in the history of astronomy, how valuable the theory of *epicycles* was, in its time: the nomenclature of the relations of a planet's orbit, which that theory introduced, was one of Kepler's resources in discovering the *elliptical* theory; and, though now superseded, is still readily intelligible to astronomers.

This is not the place to discuss the reasons for the *form* of scientific terms; otherwise we might ask, in reference to the objections to the Lavoisierian nomenclature, if such forms as *aurum* and *aura* are good to represent the absence or presence of oxygen, why such forms as *sulphite* and *sulphate* are not equally good to represent the presence of what we may call a smaller or larger dose of oxygen, so long as the oxygen theory is admitted in its present form; and to indicate still the difference of the same substances, if under any change of theory it should come to be interpreted in a new manner.

But I do not now dwell upon such arguments, my object in this place being to show that terms involving theory are not only allowable, if understood so far as the theory is proved, but of great value, and indeed of indispensable use, in science. The objection to them is inconsistent with the objects of science. If, after all that has been done in chemistry or any other science, we have

arrived at no solid knowledge, no permanent truth;—if all that we believe now may be proved to be false to-morrow;—then indeed our opinions and theories are corruptible elements, on which it would be unwise to rest any thing important, and which we might wish to exclude, even from our names. But if our knowledge has no more security than this, we can find no reason why we should wish to have names of things, since the names are needed mainly that we may reason upon and increase our knowledge such as it is. If we are condemned to endless alternations of varying opinions, then, no doubt, our theoretical terms may be a source of confusion; but then, where would be the advantage of their being otherwise? what would be the value of words which should express in a more precise manner opinions equally fleeting? It will perhaps be said, our terms must express facts, not theories: but of this distinction so applied we have repeatedly shown the futility. Theories firmly established are facts. Is it not a fact that the rusting of iron arises from the metal combining with the oxygen of the atmosphere? Is it not a fact that a combination of oxygen and hydrogen produces water? That our terms should express *such* facts, is precisely what we are here inculcating.

Our examination of the history of science has led us to a view very different from that which represents it as consisting in the succession of hostile opinions. It is, on the contrary, a progress, in which each step is recognized and employed in the succeeding one. Every theory, so far as it is true, (and all that have prevailed extensively and long, contain a large portion of truth,) is taken up into the theory which succeeds and seems to expel it. All the narrower inductions of the first are included in the more comprehensive generalizations of the second. And this is performed mainly by means of such terms

as we are now considering;—terms involving the previous theory. It is by means of such terms, that the truths at first ascertained become so familiar and manageable, that they can be employed as elementary facts in the formation of higher inductions.

These principles must be applied also, though with great caution, and in a temperate manner, even to descriptive language. Thus the mode of describing the forms of crystals adopted by Werner and Romé de l'Isle was to consider an original form, from which other forms are derived by *truncations* of the edges and the angles. Haüy's method of describing the same forms, was to consider them as built up of rows of small solids, the angles being determined by the *decrements* of these rows. Both these methods of description involve hypothetical views; and the last was intended to rest on a true physical theory of the constitution of crystals. Both hypotheses are doubtful or false: yet both these methods are good as modes of description: nor is Haüy's terminology vitiated, if we suppose (as in fact we must suppose in many instances,) that crystalline bodies are not really made up of such small solids. The mode of describing an octahedron of fluor spar, as derived from the cube, by decrements of one row on all the edges, would still be proper and useful as a description, whatever judgment we should form of the material structure of the body. But then, we must consider the solids which are thus introduced into the description as merely hypothetical geometrical forms, serving to determine the angles of the faces. It is in this way alone that Haüy's nomenclature can now be retained.

In like manner we may admit theoretical views into the descriptive phraseology of other parts of Natural History: and the theoretical terms will replace the obvious images, in proportion as the theory is generally

accepted and familiarly applied. For example, in speaking of the Honeysuckle, we may say that the upper leaves are *perfoliate*, meaning that a single orbicular leaf is perforated by the stalk, or threaded upon it. Here is an image which sufficiently conveys the notion of the form. But it is now generally recognized that this apparent single leaf is, in fact, two opposite leaves joined together at their bases. If this were doubted, it may be proved by comparing the upper leaves with the lower, which are really separate and opposite. Hence the term *connate* is applied to these conjoined opposite leaves, implying that they grow together; or they are called *connato-perfoliate*. Again; formerly the corolla was called *monopetalous* or *polypetalous*, as it consisted of one part or of several: but it is now agreed among botanists that those corollas which appear to consist of a single part, are, in fact, composed of several soldered together; hence the term *gamopetalous* is now employed (by Decandolle and his followers) instead of *monopetalous*.\*

In this way the language of Natural History not only expresses, but inevitably implies, general laws of nature; and words are thus fitted to aid the progress of knowledge in this, as in other provinces of science.

## APHORISM XII.

*If terms are systematically good, they are not to be rejected because they are etymologically inaccurate.*

TERMS belonging to a system are defined, not by the meaning of their radical words, but by their place in the system. That they should be appropriate in their signi-

\* On this subject, see Illiger, *Versuch einer Systematischen Vollständigen Terminologie für das Thierreich und Pflanzenreich*. (1810.) De Candolle, *Théorie Élémentaire de la Botanique*.



fication, aids the processes of introducing and remembering them, and should therefore be carefully attended to by those who invent and establish them; but this once done, no objections founded upon their etymological import are of any material weight. We find no inconvenience in the circumstance that *geometry* means the measuring of the earth, that the name *porphyry* is applied to many rocks which have no fiery spots, as the word implies, and *oolite* to strata which have no roelike structure. In like manner, if the term *pœcilita* were already generally received, as the name of a certain group of strata, it would be no valid ground for quarreling with it, that this group was not always variegated in colour, or that other groups were equally variegated: although undoubtedly in *introducing* such a term, care should be taken to make it as distinctive as possible. It often happens, as we have seen, that by the natural progress of changes in language, a word is steadily confirmed in a sense quite different from its etymological import. But though we may accept such instances, we must not wantonly attempt to imitate them. I say, not wantonly: for if the progress of scientific identification compel us to follow any class of objects into circumstances where the derivation of the term is inapplicable, we may still consider the term as an unmeaning sound, or rather an historical symbol, expressing a certain member of our system. Thus if, in following the course of the *mountain* or *carboniferous* limestone, we find that in Ireland it does not form mountains nor contain coal, we should act unwisely in breaking down the nomenclature in which our systematic relations are already expressed, in order to gain, in a particular case, a propriety of language which has no scientific value.

All attempts to act upon the maxim opposite to this, and to make our scientific names properly descriptive of

the objects, have failed and must fail. For the marks which really distinguish the natural classes of objects, are by no means obvious. The discovery of them is one of the most important steps in science; and when they are discovered, they are constantly liable to exceptions, because they do not contain the essential differences of the classes. The natural order *Umbellatæ*, in order to be a natural order, must contain some plants which have not umbels, as *Eryngium*\*. "In such cases," said Linnæus, "it is of small import what you *call* the order, if you take a proper series of plants, and give it some name which is clearly understood to apply to the plants you have associated." "I have," he adds, "followed the rule of borrowing the name *à fortiori*, from the principal feature."

The distinction of crystals into systems according to the degree of symmetry which obtains in them, has been explained elsewhere. Two of these systems, of which the relation as to symmetry might be expressed by saying that one is *square pyramidal* and the other *oblong pyramidal*, or the first *square prismatic* and the second *oblong prismatic*, are termed by Mohs, the first, *Pyramidal*, and the second *Prismatic*. And it may be doubted whether it is worth while to invent other terms, though these are thus defective in characteristic significance. As an example of a needless rejection of old terms in virtue of a supposed impropriety in their meaning, I may mention the attempt made in the last edition of Haüy's *Mineralogy*, to substitute *autopside* and *heteropside* for *metallic* and *unmetallic*. It was supposed to be proved that all bodies have a metal for their basis; and hence it was wished to avoid the term *unmetallic*. But the words *metallic* and *unmetallic* may mean that minerals *seem* metallic and unmetallic, just as well as

\* See *Hist. Ind. Sci.*, B. XVI. c. iv. sect. 5.

if they contained the element *opside* to imply this seeming. The old names express all that the new express, and with more simplicity, and therefore should not be disturbed.

The maxim on which we are now insisting, that we are not to be too scrupulous about the etymology of scientific terms, may, at first sight, appear to be at variance with our Fourth Aphorism, that words used technically are to retain their common meaning as far as possible. But it must be recollected, that in the Fourth Aphorism we spoke of *common* words *appropriated* as technical terms; we here speak of words *constructed* for scientific purposes. And although it is, perhaps, impossible to draw a broad line between these two classes of terms, still the rule of propriety may be stated thus: In technical terms, deviations from the usual meaning of words are bad in proportion as the words are more familiar in our own language. Thus we may apply the term *Cirrus* to a cloud composed of filaments, even if these filaments are straight; but to call such a cloud a *Curl cloud* would be much more harsh.

Since the names of things, and of classes of things, when constructed so as to involve a description, are constantly liable to become bad, the natural classes shifting away from the descriptive marks thus prematurely and casually adopted, I venture to lay down the following maxim.

### APHORISM XIII.

*The fundamental terms of a system of Nomenclature may be conveniently borrowed from casual or arbitrary circumstances\*.*

\* I may refer back to Book VIII., chap. ii., sect. 6, for some further remarks on Nomenclature. It will be seen, that besides the maxims of botanical writers concerning names, to which reference is there made

For instance, the names of plants, of minerals, and of geological strata, may be taken from the places where they occur conspicuously or in a distinct form; as *Parietaria*, *Parnassia*, *Chalcedony*, *Arragonite*, *Silurian* system, *Purbeck* limestone. These names may be considered as at first supplying standards of reference; for in order to ascertain whether any rock be *Purbeck* limestone, we might compare it with the rocks in the Isle of Purbeck. But this reference to a local standard is of authority only till the place of the object in the system, and its distinctive marks, are ascertained. It would not

some others are suggested by the considerations there offered;—especially these two:—

APHORISM XIII. (a).

The Binary method of Nomenclature (names by genus and species) is the most convenient hitherto employed in Classification.

APHORISM XIII. (b).

Numerical names in Classification are bad. For, besides that such names offer nothing for the memory to take hold of, new discoveries will probably alter the numeration, and make the names erroneous. Thus, if we call the species of a genus 1, 2, 3, &c., a new species intermediate between 1 and 2, 2 and 3, &c., cannot be put in its place without deranging the numbers.

The geological term *Trias*, lately introduced to designate the group consisting of the *three* members (Bunter Sandstein, Muschelkalk, and Keuper) becomes improper if, as some geologists hold, two of these members cannot be separated.

In like manner the names assigned by Mr. Rickman to the successive styles of Gothic architecture in England,—*Early English*, *Decorated*, and *Perpendicular*,—cannot be replaced by numerical designations, *First Pointed*, *Second Pointed*, *Third Pointed*. For—besides that he who first distinctly establishes classes has the right of naming them, and that Mr. Rickman's names are really appropriate and significant—these new names would confound all meaning of language. We should not be able to divide *Early English*, or *Decorated*, or *Perpendicular* into sub-styles;—for who could talk of *First Second Pointed* and *Second Second Pointed*; and what should we call *that* pointed style—the *Transition* from the Norman—which precedes the *First Pointed*?

vitiate the above names, if it were found that the *Parnassia* does not grow on Parnassus; that *Chalcedony* is not found in Chalcedon; or even that *Arragonite* no longer occurs in Arragon; for it is now firmly established as a mineral species. Even in geology such a reference is arbitrary, and may be superseded, or at least modified, by a more systematic determination. *Alpine* limestone is no longer accepted as a satisfactory designation of a rock, now that we know the limestone of the Alps to be of various ages.

Again, names of persons, either casually connected with the object, or arbitrarily applied to it, may be employed as designations. This has been done most copiously in botany, as for example, *Nicotiana*, *Dahlia*, *Fuchsia*, *Jungermannia*, *Lonicera*. And Linnæus has laid down rules for restricting this mode of perpetuating the memory of men, in the names of plants. Those generic names, he says\*, which have been constructed to preserve the memory of persons who have deserved well of botany, are to be religiously retained. This, he adds, is the sole and supreme reward of the botanist's labours, and must be carefully guarded and scrupulously bestowed, as an encouragement and an honour. Still more arbitrary are the terms borrowed from the names of the gods and goddesses, heroes and heroines of antiquity, to designate new genera in those departments of natural history in which so many have been discovered in recent times as to weary out all attempts at descriptive nomenclature. Cuvier has countenanced this method. "I have had to frame many new names of genera and sub-genera," he says†, "for the sub-genera which I have established were so numerous and various, that the memory is not satisfied with numerical indications. These I have chosen either so as to indicate some charac-

\* *Phil. Bot.*, 241.

† *Règne An.*, p. 16.

ter, or among the usual denominations, which I have latinized, or finally, after the example of Linnæus, among the names of mythology, which are in general agreeable to the ear, and which are far from being exhausted."

This mode of framing names from the names of persons to whom it was intended to do honour, has been employed also in the mathematical and chemical sciences; but such names have rarely obtained any permanence, except when they recorded an inventor or discoverer. Some of the constellations, indeed, have retained such appellations, as *Berenice's Hair*; and the new star which shone out in the time of Cæsar, would probably have retained the name given to it, of the *Julian Star*, if it had not disappeared again soon after. In the map of the Moon, almost all the parts have had such names imposed upon them by those who have constructed such maps, and these names have very properly been retained. But the names of new planets and satellites thus suggested have not been generally accepted; as the *Medicean* stars, the name employed by Galileo for the satellites of Jupiter, the *Georgium Sidus*, the appellation proposed by Herschel for Uranus when first discovered\*; Ceres *Ferdinanda*, the name which Piazzi wished to impose

\* In this case, the name *Uranus*, selected with a view to symmetry according to the mythological order of descent of the persons (*Uranus*, *Saturn*, *Jupiter*, *Mars*) was adopted by astronomers in general, though not proposed or sanctioned by the discoverer of the new planet. In the cases of the smaller planets, *Ceres*, *Pallas*, *Juno*, and *Vesta*, the names were given either by the discoverer, or with his sanction. Following this rule, Bessel gave the name of *Astræa* to a new planet discovered in the same region by Mr. Hencke, as mentioned in Note (N) to Book VII. of the *History* (2nd Ed.) Following the same rule, and adhering as much as possible to mythological connexion, the astronomers of Europe have, with the sanction of M. Le Verrier, given the name of *Neptune* to the planet revolving beyond Uranus, and discovered in consequence of his announcement of its probable existence, which had been inferred by

on the small planet Ceres. The names given to astronomical Tables by the astronomers who constructed them have been most steadily adhered to, being indeed names of books, and not of natural objects. Thus there were the *Ilchanic*, the *Alphonsine*, the *Rudolphine*, the *Carolinian* Tables. Comets which have been ascertained to be periodical, have very properly had assigned to them the name of the person who established this point; and of these we have thus, *Halley's*, *Encke's Comet*, and *Biela's* or *Gambart's Comet*.

In the case of discoveries in science or inventions of apparatus, the name of the inventor is very properly employed as the designation. Thus we have the *Torricellian* Vacuum, the *Voltaic* Pile, *Fahrenheit's* Thermometer. And in the same manner with regard to laws of nature, we have *Kepler's* Laws, *Boyle* or *Mariotte's* law of the elasticity of air, *Huyghens's* law of double refraction, *Newton's* scale of colours. *Descartes'* law of refraction is an unjust appellation; for the discovery of the law of sines was made by Snell. In deductive mathematics, where the invention of a theorem is generally a more definite step than an induction, this mode of designation is more common, as *Demoivre's* Theorem, *Maclaurin's* Theorem, *Lagrange's* Theorem, *Eulerian* Integrals.

In the *History of Science*\* I have remarked that in the discovery of what is termed galvanism, Volta's office was of a higher and more philosophical kind than that of Galvani; and I have, on this account, urged the propriety of employing the term *voltaic*, rather than *galvanic* electricity. I may add that the electricity of the common machine is often placed in contrast with this,

by Mr. Adams and him (calculating in ignorance of each other's purpose) from the perturbations of Uranus; as I have stated in the Preface to the Second Edition of the *History*.

\* B. XIII. c. I.

and appears to require an express name. Mr. Faraday calls it *common* or *machine* electricity; but I think that *franklinic* electricity would form a more natural correspondence with *voltæic*, and would be well justified by Franklin's place in the history of that part of the subject.

#### APHORISM XIV.

*In forming a Terminology, words may be invented when necessary, but they cannot be conveniently borrowed from casual or arbitrary circumstances\*.*

It will be recollected that Terminology is a language employed for describing objects, Nomenclature, a body of names of the objects themselves. The *names*, as was stated in the last maxim, may be arbitrary; but the *descriptive terms* must be borrowed from words of suitable meaning in the modern or the classical languages. Thus the whole terminology which Linnæus introduced into botany, is founded upon the received use of Latin words, although he defined their meaning so as to make it precise when it was not so, according to Aphorism V. But many of the terms were invented by him and other botanists, as *Perianth*, *Nectary*, *Pericarp*; so many, indeed, as to form, along with the others, a considerable language. Many of the terms which are now become familiar were originally invented by writers on botany. Thus the word *Petal*, for one division of the corolla, was introduced by Fabius Columna. The term *Sepal* was devised by Neckar to express each of the divisions of the calyx. And up to the most recent times, new deno-

\* I may also refer to B. VIII. c. ii. sect. 2, for some remarks on Terminology. The following Aphorism contains one of the most important maxims:—

#### APHORISM XIV. (a).

The meaning of Technical Terms must be fixed by convention, not by casual reference to the ordinary meaning of the words.



minations of parts and conditions of parts have been devised by botanists, when they found them necessary, in order to mark important differences or resemblances. Thus the general *Receptacle* of the flower, as it is termed by Linnæus, or *Torus*, by Salisbury, is continued into organs which carry the stamina and pistil, or the pistil alone, or the whole flower; this organ has hence been termed\* *Gonophore*, *Carpophore*, and *Anthophore*, in these cases.

In like manner when Cuvier had ascertained that the lower jaws of Saurians consisted always of six pieces having definite relations of form and position, he gave names to them, and termed them respectively the *Dental*, the *Angular*, the *Coronoid*, the *Articular*, the *Complementary*, and the *Opercular* Bones.

In all these cases, the descriptive terms thus introduced have been significant in their derivation. An attempt to circulate a perfectly arbitrary word as a means of description would probably be unsuccessful. We have, indeed, some examples approaching to arbitrary designations, in the Wernerian names of colours, which are a part of the terminology of Natural History. Many of these names are borrowed from natural resemblances, as *Auricula purple*, *Apple green*, *Straw yellow*; but the names of others are taken from casual occurrences, mostly, however, such as were already recognized in common language, as *Prussian blue*, *Dutch orange*, *King's yellow*.

The extension of arbitrary names in scientific terminology is by no means to be encouraged. I may mention a case in which it was very properly avoided. When Mr. Faraday's researches on Voltaic electricity had led him to perceive the great impropriety of the term *poles*, as applied to the apparatus, since the processes have not

\* De Candolle's *Th. El.*, 405.

reference to any opposed points, but to two opposite directions of a path, he very suitably wished to substitute for the phrases *positive pole* and *negative pole*, two words ending in *ode*, from ὁδος, a way. A person who did not see the value of our present maxim, that descriptive terms should be descriptive in their origin, might have proposed words perfectly arbitrary, as *Alphode*, and *Betode*: or, if he wished to pay a tribute of respect to the discoverers in this department of science, *Galvanode* and *Voltaode*. But such words would very justly have been rejected by Mr. Faraday, and would hardly have obtained any general currency among men of science. *Zincode* and *Platinode*, terms derived from the metal which, in one modification of the apparatus, forms what was previously termed the pole, are to be avoided, because in their origin too much is casual; and they are not a good basis for derivative terms. The pole at which the zinc is, is the Anode or Cathode, according as it is associated with different metals. Either the *Zincode* must sometimes mean the pole at which the Zinc is, and at other times that at which the Zinc is not, or else we must have as many names for poles as there are metals. *Anode* and *Cathode*, the terms which Mr. Faraday adopted, were free from these objections; for they refer to a natural standard of the direction of the voltaic current, in a manner which, though perhaps not obvious at first sight, is easily understood and retained. *Anode* and *Cathode*, the *rising* and the *setting* way, are the directions which correspond to east and west in that voltaic current to which we must ascribe terrestrial magnetism. And with these words it was easy to connect *Anion* and *Cathion*, to designate the opposite elements which are separated and liberated at the two *Electrodes*.

The following Aphorisms respect the Form of Technical Terms.

By the *Form* of Terms, I mean their philological conditions; as, for example, from what languages they may be borrowed, by what modes of inflexion they must be compounded, how their derivatives are to be formed, and the like. In this, as in other parts of the subject, I shall not lay down a system of rules, but shall propose a few maxims.

#### APHORISM XV.

*The two main conditions of the Form of technical terms are, that they must be generally intelligible, and susceptible of such grammatical relations as their scientific use requires.*

THESE conditions may at first appear somewhat vague, but it will be found that they are as definite as we could make them, without injuriously restricting ourselves. It will appear, moreover, that they have an important bearing upon most of the questions respecting the form of the words which come before us; and that if we can succeed in any case in reconciling the two conditions, we obtain terms which are practically good, whatever objections may be urged against them from other considerations.

1. The former condition, for instance, bears upon the question whether scientific terms are to be taken from the learned languages, Greek and Latin, or from our own. And the latter condition very materially affects the same question, since in English we have scarcely any power of inflecting our words; and therefore must have recourse to Greek or Latin in order to obtain terms which admit of grammatical modification. If we were content with the term *Heat* to express the science of heat, still it would be a bad technical term, for we cannot derive from it an adjective like *thermotical*. If *bed* or *layer* were an equally good term with

*stratum*, we must still retain the latter, in order that we may use the derivative *Stratification*, for which the English words cannot produce an equivalent substitute. We may retain the words *lime* and *flint*, but their adjectives for scientific purposes are not *limy* and *flinty*, but *calcareous* and *siliceous*; and hence we are able to form a compound, as *calcareo-siliceous*, which we could not do with indigenous words. We might fix the phrases *bent back* and *broken* to mean (of optical rays) that they are *reflected* and *refracted*; but then we should have no means of speaking of the angles of *Reflection* and *Refraction*, of the *Refractive* Indices, and the like.

In like manner, so long as anatomists described certain parts of a vertebra as *vertebral laminae*, or *vertebral plates*, they had no adjective whereby to signify the properties of these parts; the term *Neurapophysis*, given to them by Mr. Owen, supplies the corresponding expression *neurapophysial*. So again, the term *Basisphenoid*, employed by the same anatomist, is better than *basilar* or *basial process of the sphenoid*, because it gives us the adjective *basisphenoidal*. And the like remark applies to other changes recently proposed in the names of portions of the skeleton.

Thus one of the advantages of going to the Greek and Latin languages for the origin of our scientific terms is, that in this way we obtain words which admit of the formation of adjectives and abstract terms, of composition, and of other inflexions. Another advantage of such an origin is, that such terms, if well selected, are readily understood over the whole lettered world. For this reason, the descriptive language of science, of botany for instance, has been, for the most part, taken from the Latin; many of the terms of the mathematical and chemical sciences have been derived from the Greek; and when occasion occurs to construct a new term, it is

generally to that language that recourse is had. The advantage of such terms is, as has already been intimated, that they constitute an universal language, by means of which cultivated persons in every country may convey to each other their ideas without the need of translation.

On the other hand, the advantage of indigenous terms is, that so far as the language extends, they are intelligible much more clearly and vividly than those borrowed from any other source, as well as more easily manageable in the construction of sentences. In the descriptive language of botany, for example, in an English work, the terms *drooping*, *nodding*, *one-sided*, *twining*, *straggling*, appear better than *cernuous*, *nutant*, *secund*, *volute*, *divaricate*. For though the latter terms may by habit become as intelligible as the former, they cannot become more so to any readers; and to most English readers they will give a far less distinct impression.

2. Since the advantage of indigenous over learned terms, or the contrary, depends upon the balance of the capacity of inflexion and composition on the one hand, against a ready and clear significance on the other, it is evident that the employment of scientific terms of the one class or of the other may very properly be extremely different in different languages. The German possesses in a very eminent degree that power of composition and derivation, which in English can hardly be exercised at all, in a formal manner. Hence German scientific writers use native terms to a far greater extent than do our own authors. The descriptive terminology of botany, and even the systematic nomenclature of chemistry, are represented by the Germans by means of German roots and inflexions. Thus the description of *Potentilla anserina*, in English botanists, is that it has *Leaves interruptedly pinnate, serrate, silky, stem creeping, stalks axillar, one-*

*flowered*. Here we have words of Saxon and Latin origin mingled pretty equally. But the German description is entirely Teutonic. *Die Blume in Achsel; die Blätter unterbrochen gefiedert, die Blättchen scharf gesagt, die Stämme kriechend, die Bluthenstiele einblumig*. We could imitate this in our own language, by saying *brokenly-feathered, sharp-sawed*; by using *threed* for *ternate*, as the Germans employ *gedreit*; by saying *fingered-feathered* for *digitato-pinnate*, and the like. But the habit which we have, in common as well as scientific language, of borrowing words from the Latin for new cases, would make such usages seem very harsh and pedantic.

We may add that, in consequence of these different practices in the two languages, it is a common habit of the German reader to impose a scientific definiteness upon a common word, such as our Fifth Aphorism requires; whereas the English reader expects rather that a word which is to have a technical sense shall be derived from the learned languages. *Die Kelch* and *die Blume* (the cup and the flower) easily assume the technical meaning of *calyx* and *corolla*; *die Griffel* (the pencil) becomes *the pistil*; and a name is easily found for the *pollen*, the *anthers*, and the *stamens*, by calling them the dust, the dust-cases, and the dust-threads (*der Staub, die Staub-beutel, or Staub-fächer, and die Staub-fäden*). This was formerly done in English to a greater extent than is now possible without confusion and pedantry. Thus, in Grew's book on the *Anatomy of Plants*, the calyx is called the *impalement*, and the sepals the *impalers*; the petals are called the *leaves of the flower*; the stamens with their anthers are the *seminiform attire*. But the English language, as to such matters, is now less flexible than it was; partly in consequence of its having adopted the Linnæan terminology almost entire, without any eudeavour to naturalize it. Any attempt at idiomatic

description would interfere with the scientific language now generally received in this country. In Germany, on the other hand, those who first wrote upon science in their own language imitated the Latin words which they found in foreign writers, instead of transferring new roots into their own language. Thus the *Numerator* and *Denominator* of a fraction they call the *Namer* and the *Counter* (*Nenner* and *Zähler*). This course they pursued even where the expression was erroneous. Thus that portion of the intestines which ancient anatomists called *Duodenum*, because they falsely estimated its length at twelve inches, the Germans also term *Zwölffingerdarm* (twelve-inch-gut), though this intestine in a whale is twenty feet long, and in a frog not above twenty lines. As another example of this process in German, we may take the word *Muttersackbauchblatte*, the *uterine peritonæum*.

It is a remarkable evidence of this formative power of the German language, that it should have been able to produce an imitation of the systematic chemical nomenclature of the French school, so complete, that it is used in Germany as familiarly as the original system is in France and England. Thus Oxygen and Hydrogen are *Sauerstoff* and *Wasserstoff*; Azote is *Stickstoff* (suffocating matter); Sulphuric and Sulphurous Acid are *Schwefel-säure* and *Schwefelichte-säure*. The Sulphate and Sulphite of Baryta, and Sulphuret of Baryum, are *Schwefel-säure Baryterde*, *Schwefelichte-säure Baryterde*, and *Schwefel-baryum*. Carbonate of Iron is *Kohlen-säures Eisenoxydul*; and we may observe that, in such cases, the German name is much more agreeable to analogy than the English one; for the Protoxide of Iron, (*Eisenoxydul*), and not the Iron itself, is the base of the salt. And the German language has not only thus imitated the established nomenclature of chemistry, but has shown itself capable of supplying new forms to meet the

demands which the progress of theory occasions. Thus the Hydracids are *Wasserstoff-säuren*; and of these, the Hydriodic Acid is *Iodwasserstoff-säure*, and so of the rest. In like manner, the translator of Berzelius has found German names for the sulpho-salts of that chemist; thus he has *Wasserstoffschwefliges Schwefel-lithium*, which would be (if we were to adopt his theoretical view,) hydro-sulphuret of sulphuret of lithium: and a like nomenclature for all other similar cases.

3. In English we have no power of imitating this process, and must take our technical phrases from some more flexible language, and generally from the Latin or Greek. We are indeed so much accustomed to do this, that except a word has its origin in one of these languages, it hardly seems to us a technical term; and thus by employing indigenous terms, even descriptive ones, we may, perhaps, lose in precision more than we gain in the vividness of the impression. Perhaps it may be better to say *cuneate*, *lunate*, *hastate*, *sagittate*, *reniform*, than *wedge-shaped*, *crescent-shaped*, *halbert-headed*, *arrow-headed*, *kidney-shaped*. *Ringent* and *personate* are better than any English words which we could substitute for them; *labiate* is more precise than *lipped* would readily become. *Urceolate*, *trochlear*, are more compact than *pitcher-shaped*, *pulley-shaped*; and *infundibuliform*, *hypocrateriform*, though long words, are not more inconvenient than *funnel-shaped* and *salver-shaped*. In the same way it is better to speak (with Dr. Prichard\*) of *repent* and *progressive* animals, than of *creeping* and *progressive*: the two Latin terms make a better pair of correlatives.

4. But wherever we may draw the line between the proper use of English and Latin terms in descriptive phraseology, we shall find it advisable to borrow almost all other technical terms from the learned languages.

\* *Researches*, p. 69.



We have seen this in considering the new terms introduced into various sciences in virtue of our Ninth Maxim. We may add, as further examples, the names of the various animals of which a knowledge has been acquired from the remains of them which exist in various strata, and which have been reconstructed by Cuvier and his successors. Such are the *Palæotherium*, the *Anoplotherium*, the *Megatherium*, the *Dinotherium*, the *Chirotherium*, the *Megalichthys*, the *Mastodon*, the *Ichthyosaurus*, the *Plesiosaurus*, the *Pterodactylus*. To these others are every year added; as, for instance, very recently, the *Toxodon*, *Zeuglodon*, and *Phascolotherium* of Mr. Owen, and the *Thylacotherium* of M. Valenciennes. Still more recently the terms *Glyptodon*, *Myiodon*, *Dicynodon*, *Paloplotherium*, *Rhynchosaurus*, have been added by Mr. Owen to designate fossil animals newly determined by him.

The names of species, as well as of genera, are thus formed from the Greek: as the *Plesiosaurus dolichodeirus*, (long-necked), *Ichthyosaurus platyodon* (broad-toothed), the Irish elk, termed *Cervus megaceros* (large-horned). But the descriptive specific names are also taken from the Latin, as *Plesiosaurus brevirostris*, *longirostris*, *crassirostris*; besides which there are arbitrary specific names, which we do not here consider.

These names being all constructed at a period when naturalists were familiar with an artificial system, the standard language of which is Latin, have not been taken from modern language. But the names of living animals, and even of their classes, long ago formed in the common language of men, have been in part adopted in the systems of naturalists, agreeably to Aphorism Third. Hence the language of systems in natural history is mixed of ancient and modern languages. Thus Cuvier's divisions of the vertebrated animals are

*Mammifères* (Latin), *Oiseaux*, *Reptiles*, *Poissons*; *Bimanes*, *Quadrumanes*, *Carnassières*, *Rongeurs*, *Pachydermes* (Greek), *Ruminans* (Latin), *Cétacés* (Latin). In the subordinate divisions the distribution being more novel, the names are less idiomatic: thus the kinds of Reptiles are *Cheloniens*, *Sauriens*, *Ophidiens*, *Batraciens*, all which are of Greek origin. In like manner, Fish are divided into *Chondropterygiens*, *Malacopterygiens*, *Acanthopterygiens*. The unvertebrated animals are *Mollusques*, *Animaux articulés*, and *Animaux rayonnés*; and the Mollusques are divided into six classes, chiefly according to the position or form of their foot; namely, *Cephalopodes*, *Pteropodes*, *Gasteropodes*, *Acephales*, *Brachiopodes*, *Cirrhopodes*.

In transferring these terms into English, when the term is new in French as well as English, we have little difficulty; for we may take nearly the same liberties in English which are taken in French; and hence we may say *mammifères* (rather *mammals*), *céteans* or *cetaces*, *batraciens* (rather *batrachians*), using the words as substantives. But in other cases we must go back to the Latin: thus we say *radiate* animals, or *radiata* (rather *radials*), for *rayonnées*. These changes, however, rather refer to another Aphorism.

[Mr. Kirby has proposed *Radiary*, *Radiaries*, for *Radiata*.]

5. When new Mineral Species have been established in recent times, they have generally had arbitrary names assigned to them, derived from some person or places. In some instances, however, descriptive names have been selected; and then these have been generally taken from the Greek, as *Augite*, *Stilbite*, *Diaspore*, *Dichroite*, *Diop-tase*. Several of these Greek names imposed by Haiiy, refer to some circumstances, often fancifully selected, in his view of the crystallization of the substance, as *Epi-*

*dote, Peridote, Pleonast.* Similar terms of Greek origin have been introduced by others, as *Orthite, Anorthite, Periklin.* Greek names founded on casual circumstances are less to be commended. Berzelius has termed a mineral *Eschynite*, from αἰσχυνή, *shame*, because it is, he conceives, a shame for chemists not to have separated its elements more distinctly than they did at first.

6. In Botany, the old names of genera of Greek origin are very numerous, and many of them are descriptive, as *Glycyrhiza* (γλυκὺς and ῥίζα, sweet root) liquorice, *Rhododendron* (rose-tree), *Hæmatoxylon* (bloody wood), *Chrysocoma* (golden hair), *Alopecurus* (fox-tail), and many more. In like manner there are names which derive a descriptive significance from the Latin, either adjectives, as *Impatiens, Gloriosa, Sagittaria*, or substantives irregularly formed, as *Tussilago* (à tussis domatione), *Urtica* (ab urendo tactu), *Salsola* (à salsedine). But these, though good names when they are established by tradition, are hardly to be imitated in naming new plants. In most instances, when this is to be done, arbitrary or local names have been selected, as *Strelitzia*.

7. In Chemistry, new substances have of late had names assigned them from Greek roots, as *Iodine*, from its violet colour, *Chlorine* from its green colour. In like manner fluorine has by the French chemists been called *Phthor*, from its destructive properties. So the new metals, *Chrome, Rhodium, Iridium, Osmium*, had names of Greek derivation descriptive of their properties. Some such terms, however, were borrowed from localities, as *Strontia, Yttria*, the names of new earths. Others have a mixed origin, as *Pyrogallic, Pyroacetic*, and *Pyroligneous Spirit*. In some cases the deviation has been extravagantly capricious. Thus in the process for making Pyrogallic Acid, a certain substance is left behind, from which M. Braconnot extracted an acid

which he called *Ellagic Acid*, framing the root of the name by reading the word *Galle* backwards.

The new laws which the study of Electro-chemistry brought into view, required a new terminology to express their conditions: and in this case, as we have observed in speaking of the Twelfth Maxim, arbitrary words are less suitable. Mr. Faraday very properly borrowed from the Greek his terms *Electrolyte*, *Electrode*, *Anode*, *Cathode*, *Anion*, *Cathion*, *Dielectric*. In the mechanico-chemical and mechanical sciences, however, new terms are less copiously required than in the sciences of classification, and when they are needed, they are generally determined by analogy from existing terms. *Thermo-electricity* and *Electro-dynamics* were terms which very naturally offered themselves; Nobili's *thermo-multiplier*, Snow Harris's *unit-jar*, were almost equally obvious names. In such cases, it is generally possible to construct terms both compendious and descriptive, without introducing any new radical words.

8. The subject of Crystallography has inevitably given rise to many new terms, since it brings under our notice a great number of new relations of a very definite but very complex form. Häüy attempted to find names for all the leading varieties of crystals, and for this purpose introduced a great number of new terms, founded on various analogies and allusions. Thus the forms of calc-spar are termed by him *primitive*, *equiaxe*, *inverse*, *metastatique*, *contrastante*, *imitable*, *birhomboidale*, *prismatique*, *apophane*, *uniternaire*, *bisunitaire*, *dodécaèdre*, *contractée*, *dilatée*, *sexduodecimale*, *bisalterne*, *binoter-naire*, and many others. The want of uniformity in the origin and scheme of these denominations would be no valid objection to them, if any general truth could be expressed by means of them: but the fact is, that there is no definite distinction of these forms. They pass into

each other by insensible gradations, and the optical and physical properties which they possess are common to all of them. And as a mere enunciation of laws of form, this terminology is insufficient. Thus it does not at all convey the relation between the *bisalterne* and the *bino-ternaire*, the former being a combination of the *metastatique* with the *prismatique*, the latter, of the *metastatique* with the *contrastante*: again, the *contrastante*, the *mixte*, the *cuboïde*, the *contractée*, the *dilatée*, all contain faces generated by a common law, the index being respectively altered so as to be in these cases, 3,  $\frac{3}{2}$ ,  $\frac{4}{3}$ ,  $\frac{2}{3}$ ,  $\frac{5}{4}$ ; and this, which is the most important geometrical relation of these forms, is not at all recorded or indicated by the nomenclature. The fact is, that it is probably impossible, the subject of crystallography having become so complex as it now is, to devise a system of names which shall express the relations of form. Numerical symbols, such as those of Weiss or Naumann, or Professor Miller, are the proper ways of expressing these relations, and are the only good crystallographic terminology for cases in detail.

The terms used in expressing crystallographic laws have been for the most part taken from the Greek by all writers except some of the Germans. These, we have already stated, have constructed terms in their own language, as *zwei-und-ein gliedrig*, and the like.

In Optics we have some new terms connected with crystalline laws, as *uniaxal* and *biaxal* crystals, *optical axes*, which offered themselves without any effort on the part of the discoverers. In the whole history of the undulatory theory, very few innovations in language were found necessary, except to fix the sense of a few phrases, as *plane-polarized* light in opposition to *circularly-polarized*, and the like.

This is still more the case in Mechanics, Astronomy,

and pure mathematics. In these sciences, several of the primary stages of generalization being already passed over, when any new steps are made, we have before us some analogy by which we may frame our new terms. Thus when the *plane of maximum areas* was discovered, it had not some new arbitrary denomination assigned it, but the name which obviously described it was fixed as a technical name.

The result of this survey of the scientific terms of recent formation seems to be this;—that indigenous terms may be employed in the descriptions of facts and phenomena as they at first present themselves; and in the first induction from these; but that when we come to generalize and theorize, terms borrowed from the learned languages are more readily fixed and made definite, and are also more easily connected with derivatives. Our native terms are more impressive, and at first more intelligible; but they may wander from their scientific meaning, and are capable of little inflexion. Words of classical origin are precise to the careful student, and capable of expressing, by their inflexions, the relations of general ideas; but they are unintelligible, even to the learned man, without express definition, and convey instruction only through an artificial and rare habit of thought.

Since in the balance between words of domestic and of foreign origin so much depends upon the possibility of inflexion and derivation, I shall consider a little more closely what are the limits and considerations which we have to take into account in reference to that subject.

#### APHORISM XVI.

*In the composition and inflexion of technical terms, philological analogies are to be preserved if possible, but modified according to scientific convenience.*

IN the language employed or proposed by writers upon subjects of science, many combinations and forms of derivation occur, which would be rejected and condemned by those who are careful of the purity and correctness of language. Such anomalies are to be avoided as much as possible; but it is impossible to escape them altogether, if we are to have a scientific language which has any chance of being received into general use. It is better to admit compounds which are not philologically correct, than to invent many new words, all strange to the readers for whom they are intended: and in writing on science in our own language, it is not possible to avoid making additions to the vocabulary of common life; since science requires exact names for many things which common language has not named. And although these new names should, as much as possible, be constructed in conformity with the analogies of the language, such extensions of analogy can hardly sound, to the grammarian's ear, otherwise than as solecisms. But, as our maxim indicates, the analogy of science is of more weight with us than the analogy of language: and although anomalies in our phraseology should be avoided as much as possible, innovations must be permitted wherever a scientific language, easy to acquire, and convenient to use, is unattainable without them.

I shall proceed to mention some of the transgressions of strict philological rules, and some of the extensions of grammatical forms, which the above conditions appear to render necessary.

1. The combination of different languages in the derivation of words, though to be avoided in general, is in some cases admissible.

Such words are condemned by Quintilian and other grammarians, under the name of *hybrids*, or things of a mixed race; as *biclinium*, from *bis* and κλίνη; *epitogium*,

from *eri* and *toga*. Nor are such terms to be unnecessarily introduced in science. Whenever a homogeneous word can be formed and adopted with the same ease and convenience as a hybrid, it is to be preferred. Hence we must have *ichthyology*, not *piscology*, *entomology*, not *insectology*, *insectivorous*, not *insectophagous*. In like manner, it would be better to say *unoculus* than *monoculus*, though the latter has the sanction of Linnæus, who was a purist in such matters. Dr. Turner, in his *Chemistry*, speaks of *protoxides* and *binoxides*, which combination violates the rule for making the materials of our terms as homogeneous as possible; *protoxide* and *deutoxide* would be preferable, both on this and on other accounts.

Yet this rule admits of exceptions. *Mineralogy*, with its Greek termination, has for its root *minera*, a mediæval Latin word of Teutonic origin, and is preferable to *Oryctology*. *Terminology* appears to be better than *Glossology*: which according to its derivation would be rather the science of language in general than of technical terms; and *Horology*, from *ὥρος*, a term, would not be immediately intelligible, even to Greek scholars; and is already employed to indicate the science which treats of horologes, or time-pieces.

Indeed, the English reader is become quite familiar with the termination *ology*, the names of a large number of branches of science and learning having that form. This termination is at present rather apprehended as a formative affix in our own language, indicating a science, than as an element borrowed from a foreign language. Hence, when it is difficult or impossible to find a Greek term which clearly designates the subject of a science, it is allowable to employ some other, as in *Tidology*, the doctrine of the Tides.

The same remark applies to some other Greek ele-



ments of scientific words: they are so familiar to us that in composition they are almost used as part of our own language. This naturalization has taken place very decidedly in the element *arch*, (*ἀρχός*, a leader,) as we see in *archbishop*, *archduke*. It is effected in a great degree for the preposition *anti*: thus we speak of *anti-slavery* societies, *anti-reformers*, *anti-bilious*, or *anti-acid* medicines, without being conscious of any anomaly. The same is the case with the Latin preposition *præ* or *pre*, as appears from such words as *pre-engage*, *pre-arrange*, *pre-judge*, *pre-paid*; and in some measure with *pro*, for in colloquial language we speak of *pro-catholics* and *anti-catholics*. Also the preposition *ante* is similarly used, as *ante-nicene* fathers. The preposition *co*, abbreviated from *con*, and implying things to be simultaneous or connected, is firmly established as part of the language, as we see in *coexist*, *coheir*, *coordinate*; hence I have called those lines *cotidal* lines which pass through places where the high water of the tide occurs simultaneously.

2. As in the course of the mixture by which our language has been formed, we have thus lost all habitual consciousness of the difference of its ingredients, (Greek, Latin, Norman-French, and Anglo-Saxon): we have also ceased to confine to each ingredient the mode of grammatical inflexion which originally belonged to it. Thus the termination *ive* belongs peculiarly to Latin adjectives, yet we say *sportive*, *talkative*. In like manner, *able* is added to words which are not Latin, as *eatable*, *drinkable*, *pitiable*, *enviable*. Also the termination *al* and *ical* are used with various roots, as *loyal*, *royal*, *farcical*, *whimsical*; hence we may make the adjective *tidal* from tide. This ending, *al*, is also added to abstract terms in *ion*, as *occasional*, *provisional*, *intentional*, *national*; hence we may, if necessary, use such words as *educa-*

*tional, terminational.* The ending *ic* appears to be suited to proper names, as *Pindaric, Socratic, Platonic*; hence it may be used when scientific words are derived from proper names, as *Voltaic* or *Galvanic* electricity: to which I have proposed to add *Franklinic*.

In adopting scientific adjectives from the Latin, we have not much room for hesitation; for, in such cases, the habits of derivation from that language into our own are very constant; *ivus* becomes *ive*, as *decursive*; *inus* becomes *ine*, as in *ferine*; *atus* becomes *ate*, as *hastate*; and *us* often becomes *ous*, as *rufous*; *aris* becomes *ary*, as *axillary*; *ens* becomes *ent*, as *ringent*. And in adopting into our language, as scientific terms, words which in another language, the French for instance, have a Latin origin familiar to us, we cannot do better than form them as if they were derived directly from the Latin. Hence the French adjectives *cétacé, crustacé, testacé*, may become either *cetaceous, crustaceous, testaceous*, according to the analogy of *farinaceous, predaceous*, or else *cetacean, crustacean, testacean*, imitating the form of *patrician*. Since, as I shall soon have to notice, we require substantives as well as adjectives from these words, we must, at least for that use, take the forms last suggested.

In pursuance of the same remark, *rongeur* becomes *rodent*; and *edenté* would become *edentate*, but that this word is rejected on another account: the adjectives *bimane* and *quadrumane* are *bimanous* and *quadrumanous*.

3. There is not much difficulty in thus forming adjectives: but the purposes of Natural History require that we should have substantives corresponding to these adjectives; and these cannot be obtained without some extension of the analogies of our language. We cannot in general use adjectives or participles as singu-

lar substantives. *The happy* or *the doomed* would, according to good English usage, signify those who are happy and those who are doomed. Hence we could not speak of a particular scaled animal as *the squamate*, and still less could we call any such animal *a squamate*, or speak of *squamates* in the plural. Some of the forms of our adjectives, however, do admit of this substantive use. Thus we talk of *Europeans*, *plebeians*, *republicans*; of *divines* and *masculines*; of the *ultramontanes*; of *mor-dants* and *brilliantes*; of *abstergents* and *emollients*; of *mercenaries* and *tributaries*; of *animals*, *manuals*, and *officials*; of *dissuasives* and *motives*. We cannot generally use in this way adjectives in *ous*, nor in *ate* (though *reprobates* is an exception), nor English participles, nor adjectives in which there is no termination imitating the Latin, as *happy*, *good*. Hence, if we have, for purposes of science, to convert adjectives into substantives, we ought to follow the form of examples like these, in which it has already appeared in fact, that such usage, though an innovation at first, may ultimately become a received part of the language.

By attention to this rule we may judge what expressions to select in cases where substantives are needed. I will take as an example the division of the mammalian animals into Orders. These Orders, according to Cuvier, are *Bimanes*, *Quadrumanes*, *Carnassiers*, *Rongeurs*, *Edentés*, *Ruminans*, *Pachydermes*, *Cétacés*; and of these, *Bimanes*, *Quadrumanes*, *Rodents*, *Ruminants*, *Pachyderms* are admissible as English substantives on the grounds just stated. *Cetaceous* could not be used substantively; but *Cetacean* in such a usage is sufficiently countenanced by such cases as we have mentioned, *patri-cian*, &c.; hence we adopt this form. We have no English word equivalent to the French *Carnassiers*: the English translator of Cuvier has not provided English

words for his technical terms; but has formed a Latin word, *Carnaria*, to represent the French terms. From this we might readily form *Carnaries*; but it appears much better to take the Linnæan name *Feræ* as our root, from which we may take *Ferine*, substantive as well as adjective; and hence we call this order *Ferines*. The word for which it is most difficult to provide a proper representation, is *Edenté*, *Edentata*: for, as we have said, it would be very harsh to speak of the order as the *Edentates*; and if we were to abbreviate the word into *edent*, we should suggest a false analogy with *rodent*, for as *rodent* is *quod rodit*, that which gnaws, *edent* would be *quod edit*, that which eats. And even if we were to take *edent* as a substantive, we could hardly use it as an adjective: we should still have to say, for example, the *edentate* form of head. For these reasons it appears best to alter the form of the word, and to call the Order the *Edentals*, which is quite allowable, both as adjective and substantive.

[An objection might be made to this term, both in its Latin, French and English form: namely, that the natural group to which it is applied includes many species, both existing and extinct, well provided with teeth. Thus the armadillo is remarkable for the number of its teeth; the megatherium, for their complex structure. But the analogy of scientific language readily permits us to fix, upon the word *edentata*, a special meaning, implying the absence of one particular kind of teeth, namely, incisive teeth. Linnæus called the equivalent order *Bruta*. We could not apply in this case the term *Brutes*; for common language has already attached to the word a wider meaning, too fixedly for scientific use to trifle with it.]

There are several other words in *ate* about which there is the same difficulty in providing substantive

forms. Are we to speak of *Vertebrates*? or would it not be better, in agreement with what has been said above, to call these *Vertebrals*, and the opposite class *Invertebrals*?

There are similar difficulties with regard to the names of subordinate portions of zoological classification; thus the Ferines are divided by Cuvier into *Cheiroptères*, *Insectivores*, *Carnivores*; and these latter into *Plantigrades*, *Digitigrades*, *Amphibies*, *Marsupiaux*. There is not any great harshness in naturalizing these substantives as *Chiropters*, *Insectivores*, *Carnivores*, *Plantigrades*, *Digitigrades*, *Amphibians*, and *Marsupials*. These words *Carnivores* and *Insectivores* are better, because of more familiar origin, than Greek terms; otherwise we might, if necessary, speak of *Zoophagans* and *Entomophagans*.

It is only with certain familiar adjectival terminations, as *ous* and *ate*, that there is a difficulty in using the word as substantive. When this can be avoided, we readily accept the new word, as *Pachyderms*, and in like manner *Mollusks*.

If we examine the names of the Orders of Birds, we find that they are in Latin, *Predatores* or *Accipitres*, *Passeres*, *Scansores*, *Rasores* or *Gallinæ*, *Grallatores*, *Palmipedes* and *Anseres*: Cuvier's Orders are, *Oiseaux de Proie*, *Passereaux*, *Grimpeurs*, *Gallinacés*, *Echasiérs*, *Palmipedes*. These may be englished conveniently as *Predators*, *Passerines*, *Scansors*, *Gallinaceans*, (rather than *Rasors*,) *Grallators*, *Palmipedans*, [or rather *Palmipeds*, like *Bipeds*]. *Scansors*, *Grallators*, and *Rasors*, are better, as technical terms, than *Climbers*, *Waders*, and *Scratchers*. We might venture to anglicize the terminations of the names which Cuvier gives to the divisions of these Orders: thus the *Predators* are the *Diurnals* and the *Nocturnals*; the *Passerines* are

the *Dentirostres*, the *Fissirostres*, the *Conirostres*, the *Tenuirostres*, and the *Syndactyls*: the word *lustre* showing that the former termination is allowable. The Scansors are not sub-divided, nor are the Gallinaceans. The Grallators are *Pressirostres*, *Cultriostres*, *Macro-dactyls*. The Palmipeds are the *Plungers*, the *Longipens*, the *Totipalmes* and the *Lamellirostres*.

The next class of Vertebrals is the *Reptiles*, and these are either *Chelonians*, *Saurians*, *Ophidians*, or *Batrachians*. Cuvier writes *Batraciens*, but we prefer the spelling to which the Greek word directs us.

The last or lowest class is the *Fishes*, in which province Cuvier has himself been the great systematist, and has therefore had to devise many new terms. Many of these are of Greek or Latin origin, and can be anglicized by the analogies already pointed out, as *Chondropterygians*, *Malacopterygians*, *Lophobranchs*, *Plectognaths*, *Gymnodonts*, *Scleroderms*. *Discoboles* and *Apodes* may be English as well as French. There are other cases in which the author has formed the names of Families, either by forming a word in *ides* from the name of a genus, as *Gadoides*, *Gobiöides*, or by gallicizing the Latin name of the genus, as *Salmones* from *Salmo*, *Clupes* from *Clupea*, *Esoces* from *Esox*, *Cyprins* from *Cyprinus*. In these cases Agassiz's favourite form of names for families of fishes has led English writers to use the words *Gadoids*, *Gobioids*, *Salmonoids*, *Clupeoids*, *Lucioids* (for *Esocés*) *Cyprinoids*, &c. There is a taint of hybridism in this termination, but it is attended with this advantage, that it has begun to be characteristic of the nomenclature of family groups in the class *Pisces*. One of the orders of fishes, co-ordinate with the Chondropterygians and the Lophobranchs, is termed *Osseux* by Cuvier. It appears hardly worth while to invent a substantive word for this,

when *Bony Fishes* is so simple a phrase, and may readily be understood as a technical name of a systematic order.

The Mollusks are the next Class; and these are divided into *Cephalopods*, *Gasteropods*, and the like. The Gasteropods are *Nudibranchs*, *Inferobranchs*, *Tectibranchs*, *Pectinibranchs*, *Scutibranchs*, and *Cyclobranchs*. In framing most of these terms Cuvier has made hybrids by a combination of a Latin word with *branchiæ*, which is the Greek name for the gills of a fish; and has thus avoided loading the memory with words of an origin not obvious to most naturalists, as terms derived from the Greek would have been. Another division of the Gasteropods is *Pulmonés*, which we must make *Pulmonians*. In like manner the subdivisions of the Pectinibranchs are the *Trochoidans* and *Buccinoidans*, (*Trochoïdes*, *Buccinoïdes*). The *Acéphales*, another order of Mollusks, may be *Acephals* in English.

After these comes the third grand division, *Articulated Animals*, and these are *Annelidans*, *Crustaceans*, *Arachnidans*, and *Insects*. I shall not dwell upon the names of these, as the form of English words which is to be selected must be sufficiently obvious from the preceding examples.

Finally, we have the fourth grand division of animals, the *Rayonnés*, or *Radiata*; which, for reasons already given, we may call *Radials*, or *Radiaries*. These are *Echinoderms*, *Intestinals*, (or rather *Entozoans*,) *Acalephes*, and *Polyps*. The Polyps, which are composite animals in which many gelatinous individuals are connected so as to have a common life, have, in many cases, a more solid framework belonging to the common part of the animal. This framework, of which coral is a special example, is termed in French *Polypier*; the word has been angli-

cized by the word *polypary*, after the analogy of *aviary* and *apiary*. Thus Polyps are either *Polyps with Polyparies* or *Naked Polyps*.

Any common kind of Polyps has usually in the English language been called *Polypus*, the Greek termination being retained. This termination in *us*, however, whether Latin or Greek, is to be excluded from the English as much as possible, on account of the embarrassment which it occasions in the formation of the plural. For if we say *Polypi* the word ceases to be English, while *Polypuses* is harsh: and there is the additional inconvenience, that both these forms would indicate the plural of individuals rather than of classes. If we were to say, "The Corallines are a Family of the *Polypuses with Polyparies*," it would not at once occur to the reader that the three last words formed a technical phrase.

This termination *us*, which must thus be excluded from the names of families, may be admitted in the designation of genera; of animals, as *Nautilus*, *Echinus*, *Hippopotamus*; and of plants, as *Crocus*, *Asparagus*, *Narcissus*, *Acanthus*, *Ranunculus*, *Fungus*. The same form occurs in other technical words, as *Fucus*, *Mucus*, *Œsophagus*, *Hydrocephalus*, *Callus*, *Calculus*, *Uterus*, *Fœtus*, *Radius*, *Focus*, *Apparatus*. It is, however, advisable to retain this form only in cases where it is already firmly established in the language; for a more genuine English form is preferable. Hence we say, with Mr. Lyell, *Ichthyosaur*, *Plesiosaur*, *Pterodactyl*. In like manner Mr. Owen anglicizes the termination *erium*, and speaks of the *Anoplothere* and *Paleothere*.

Since the wants of science thus demand adjectives which can be used also as substantive names of classes, this consideration may sometimes serve to determine our selection of new terms. Thus Mr. Lyell's names for the



subdivisions of the tertiary strata, *Miocene*, *Pliocene*, can be used as substantives; but if such words as *Mioneous*, *Plioneous*, had suggested themselves, they must have been rejected, though of equivalent signification, as not fulfilling this condition.

4. (a.) Abstract substantives can easily be formed from adjectives: from electric we have *electricity*; from galvanic, *galvanism*; from organic, *organization*; *velocity*, *levity*, *gravity*, are borrowed from Latin adjectives. *Caloric* is familiarly used for the matter of heat, though the form of the word is not supported by any obvious analogy.

(b.) It is quite intolerable to have words regularly formed, in opposition to the analogy which their meaning offers; as when bodies are said to have *conductibility* or *conducibility* with regard to heat. The bodies are *conductive*, and their property is *conductivity*.

(c.) The terminations *ize* (rather than *ise*), *ism*, and *ist*, are applied to words of all origins: thus we have to *pulverize*, to *colonize*, *Witticism*, *Heathenism*, *Journalist*, *Tobacconist*. Hence we may make such words when they are wanted. As we cannot use *physician* for a cultivator of physics, I have called him a *Physicist*. We need very much a name to describe a cultivator of science in general. I should incline to call him a *Scientist*. Thus we might say, that as an Artist is a Musician, Painter, or Poet, a Scientist is a Mathematician, Physicist, or Naturalist.

(d.) Connected with verbs in *ize*, we have abstract nouns in *ization*, as *polarization*, *crystallization*. These it appears proper to spell in English with *z* rather than *s*; governing our practice by the Greek verbal termination  $\iota\zeta\omega$  which we imitate. But we must observe that verbs and substantives in *yse*, (*analyse*), belong to a

different analogy, giving an abstract noun in *ysis* and an adjective *ytic* or *ytical*; (*analysis*, *analytic*, *analytical*). Hence *electrolyse* is more proper than *electrolyze*\*.

(e.) The names of many sciences end in *ics* after the analogy of *Mathematics*, *Metaphysics*; as *Optics*, *Mechanics*. But these in most other languages, as in our own formerly, have the singular form *Optice*, *l'Optique*, *Optik*, *Optick*: and though we now write *Optics*, we make such words of the singular number: "Newton's Opticks is an example." As, however, this connexion in new words is startling, as when we say "Thermo-electrics is now much cultivated," it appears better to employ the singular form, after the analogy of *Logic* and *Rhetoric*, when we have words to construct. Hence we may call the science of languages *Linguistic*, as it is called by the best German writers, for instance, William von Humboldt.

5. In the derivation of English from Latin or Greek words, the changes of letters are to be governed by the rules which have generally prevailed in such cases. The Greek *α* and *αι*, the Latin *oe* and *ae*, are all converted into a simple *e*, as in *Economy*, *Geodesy*, *penal*, *Cesar*. Hence, according to common usage, we should write *phenomena*, not *phænomena*, *paleontology*, not *palæontology*, *miocene* not *miocæne*, *pæklite* not *pœklite*. But in order to keep more clearly in view the origin of our terms, it may be allowable to deviate from these rules of change, especially so long as the words are still new and unfamiliar. Dr. Buckland speaks of the *poikilitic*, not *pecilitic*, group of strata: *palæontology* is the spelling commonly adopted; and in imitation of this I have written *palætiology*. The diphthong *ei* was by the Latins changed into *i*, as in *Aristides*; and hence this has been the usual

\* I fear I have, in some of the preceding pages, neglected this distinction.

form in English. Some recent authors indeed (Mr. Mitford for instance) write *Aristeides*; but the former appears to be the more legitimate. Hence we write *miocene*, *pliocene*, not *meiocene*, *pleiocene*. The Greek  $\upsilon$  becomes *y*, and  $\omicron\upsilon$  becomes *u*, in English as in Latin, as *crystal*, *colure*. The consonants  $\kappa$  and  $\chi$  become *c* and *ch* according to common usage. Hence we write *crystal*, not *chrysal*, *batrachian*, not *batracian*, *cryolite*, not *chryolite*. As, however, the letter *c* before *e* and *i* differs from *k*, which is the sound we assign to the Greek  $\kappa$ , it may be allowable to use *k* in order to avoid this confusion. Thus, as we have seen, *poikilite* has been used, as well as *pecilite*. Even in common language some authors write *skeptic*, which appears to be better than *sceptic* with our pronunciation, and is preferred by Dr. Johnson. For the same reason, namely to avoid confusion in the pronunciation, and also, in order to keep in view the connexion with *cathode*, the elements of an electrolyte which go to the anode and cathode respectively may be termed the anion and cathion; although the Greek would suggest cation, ( $\kappa\alpha\tau\acute{\iota}\omicron\nu$ ).

6. The example of chemistry has shown that we have in the terminations of words a resource of which great use may be made in indicating the relations of certain classes of objects: as *sulphurous* and *sulphuric* acids; *sulphates*, *sulphites*, and *sulphurets*. Since the introduction of the artifice by the Lavoisierian school, it has been extended to some new cases. The *Chlorine*, *Fluorine*, *Bromine*, *Iodine*, had their names put into that shape in consequence of their supposed analogy: and for the same reason have been termed *Chlore*, *Phtore*, *Brome*, *Iode*, by French chemists. In like manner, the names of metals in their Latin form have been made to end in *um*, as *Osmium*, *Palladium*; and hence it is better to say *Platinum*, *Molybdenum*, than *Platina*, *Molyb-*

*dena*. It has been proposed to term the basis of Boracic acid *Boron*; and those who conceive that the basis of Silica has an analogy with Boron have proposed to term it *Silicon*, while those who look upon it as a metal would name it *Silicium*. *Selenium* was so named when it was supposed to be a metal: as its analogies are now acknowledged to be of another kind, it would be desirable, if the change were not too startling, to term it *Selen*, as it is in German. *Phosphorus* in like manner might be *Phosphur*, which would indicate its analogy with *Sulphur*.

The resource which terminations offer has been applied in other cases. The names of many species of minerals end in *lite*, or *ite*, as *Staurolite*, *Augite*. Hence Adolphe Brongniart, in order to form a name for a genus of fossil plants, has given this termination to the name of the recent genus which they nearly resemble, as *Zamites* from *Zamia*, *Lycopodites* from *Lycopodium*.

Names of different genera which differ in termination only are properly condemned by Linnæus\*; as *Alsine*, *Alsinoides*, *Alsinella*, *Alsinastrum*; for there is no definite relation marked by those terminations. Linnæus gives to such genera distinct names, *Alsine*, *Bufonia*, *Sagina*, *Elatine*.

Terminations are well adapted to express definite systematic relations, such as those of chemistry, but they must be employed with a due regard to all the bearings of the system. Davy proposed to denote the combinations of other substances with chlorine by peculiar terminations; using *ane* for the smallest proportion of Chlorine, and *anea* for the larger, as *Cuprane*, *Cupranea*. In this nomenclature, common salt would be *Sodane*, and Chloride of Nitrogen would be *Azotane*. This suggestion never found favour. It was objected that it was

\* *Phil. Bot.*, 231.

contrary to the Linnæan precept, that a specific name must not be united to a generic as a termination. But this was not putting the matter exactly on its right ground; for the rules of nomenclature of natural history do not apply to chemistry; and the Linnæan rule might with equal propriety have been adduced as a condemnation of such terms as *Sulphurous*, *Sulphuric*. But Davy's terms were bad; for it does not appear that Chlorine enters, as Oxygen does, into so large a portion of chemical compounds, that its relations afford a key to their nature, and may properly be made an element in their names.

This resource, of terminations, has been abused, wherever it has been used wantonly, or without a definite significance in the variety. This is the case in M. Beudant's Mineralogy. Among the names which he has given to new species, we find the following (besides many in *ite*), *Scolexerose*, *Opsimose*, *Exanthelose*, &c.; *Diacrase*, *Panabase*, *Neoplase*; *Neoclese*; *Rhodoise*, *Stibiconise*, &c.; *Marceline*, *Whilelmine*, &c.; *Exitele*, and many others. In addition to other objections which might be made to these names, their variety is a material defect: for to make this variety depend on caprice alone, as in those cases it does, is to throw away a resource of which chemical nomenclature may teach us the value.

#### APHORISM XVII.

*When alterations in technical terms become necessary, it is desirable that the new term should contain in its form some memorial of the old one.*

WE have excellent examples of the advantageous use of this maxim in Linnæus's reform of botanical nomenclature. His innovations were very extensive, but they

were still moderated as much as possible, and connected in many ways with the names of plants then in use. He has himself given several rules of nomenclature, which tend to establish this connexion of the old and new in a reform. Thus he says, "Generic names which are current, and are not accompanied with harm to botany, should be tolerated\*." "A passable generic name is not to be changed for another, though more apt†." "New generic names are not to be framed so long as passable synonyms are at hand‡." "A generic name of one genus, except it be superfluous, is not to be transferred to another genus, though it suit the other better§." "If a received genus requires to be divided into several, the name which before included the whole, shall be applied to the most common and familiar kind||." And though he rejects all *generic* names which have not a Greek or Latin root¶, he is willing to make an exception in favour of those which from their form might be supposed to have such a root, though they are really borrowed from other languages, as *Thea*, which is the Greek for goddess; *Coffea*, which might seem to come from a Greek word denoting silence (κωφός); *Cheiranthus*, which appears to mean hand-flower, but is really derived from the Arabic *Keiri*: and many others.

As we have already said, the attempt at a reformation of the nomenclature of Mineralogy made by Professor Mohs will probably not produce any permanent effect, on this account amongst others, that it has not been conducted in this temperate mode; the innovations bear too large a proportion to the whole of the names, and contain too little to remind us of the known appellations. Yet in some respects Professor Mohs has acted upon this maxim. Thus he has called one of his classes

\* *Philosophia Botanica*, Art. 242.

† P. 246.

‡ *Phil. Bot.*, p. 247. § P. 249.

|| P. 249. ¶ P. 232.

*Spar*, because *Felspar* belongs to it. I shall venture to offer a few suggestions on this subject of Mineralogical Nomenclature.

It has already been remarked that the confusion and complexity which prevail in this subject render a reform very desirable. But it will be seen, from the reasons assigned under the Ninth Aphorism, that no permanent system of names can be looked for, till a sound system of classification be established. The best mineralogical systems recently published, however, appear to converge to a common point; and certain classes have been formed which have both a natural-historical and a chemical significance. These Classes, according to Naumann, whose arrangement appears the best, are Hydrolytes, Haloids, Silicides, Oxides of Metals, Metals, Sulphurides (Pyrites, Glances, and Blendes), and Anthracides. Now we find;—that the Hydrolytes are all compounds, such as are commonly termed *Salts*;—that the Haloids are, many of them, already called *Spars*, as *Calc Spar*, *Heavy Spar*, *Iron Spar*, *Zinc Spar*;—that the *Silicides*, the most numerous and difficult class, are denoted for the most part, by single words, many of which end in *ite*;—that the other classes, or sub-classes, *Oxides*, *Pyrites*, *Glances*, and *Blendes*, have commonly been so termed; as *Red Iron Oxide*, *Iron Pyrites*, *Zinc Blende*;—while pure metals have usually had the adjective *native* prefixed, as *Native Gold*, *Native Copper*. These obvious features of the current names appear to afford us a basis for a systematic nomenclature. The Salts and Spars might all have the word *salt* or *spar* included in their name, as *Natron Salt*, *Glauber Salt*, *Rock Salt*; *Calc Spar*, *Bitter Spar*, (Carbonate of Lime and Magnesia), *Fluor Spar*, *Phosphor Spar*, (Phosphate of Lime), *Heavy Spar*, *Celestine Spar* (Sulphate of Strontian), *Chromic Lead Spar* (Chromate of Lead); the *Silicides*

might all have the name constructed so as to be a single word ending in *ite*, as *Chabasite* (Chabasie), *Natrolite* (Mesotype), *Sommite* (Nepheline), *Pistacite* (Epidote); from this rule might be excepted the *Gems*, as *Topaz*, *Emerald*, *Corundum*, which might retain their old names. The Oxides, Pyrites, Glances, and Blendes, might be so termed; thus we should have *Tungstic Iron Oxide* (usually called Tungstate of Iron), *Arsenical Iron Pyrites* (Mispickel), *Tetrahedral Copper Glance* (Fahlerz), *Quicksilver Blende* (Cinnabar), and the Metals might be termed *native*, as *Native Copper*, *Native Silver*.

Such a nomenclature would take in a very large proportion of commonly received appellations, especially if we were to select among the synonyms, as is proposed above in the case of *Glauber Salt*, *Bitter Spar*, *Sommite*, *Pistacite*, *Natrolite*. Hence it might be adopted without serious inconvenience. It would make the name convey information respecting the place of the mineral in the system; and by imposing this condition, would limit the extreme caprice, both as to origin and form, which has hitherto been indulged in imposing mineralogical names.

The principle of a mineralogical nomenclature determined by the place of the species in the system, has been recognized by Mr. Beudant as well as Mr. Mohs. The former writer has proposed that we should say *Carbonate Calcaire*, *Carbonate Witherite*, *Sulphate Couperose*, *Silicate Stilbite*, *Silicate Chabasie*, and so on. But these are names in which the part added for the sake of the system, is not incorporated with the common name, and would hardly make its way into common use.

We have already noticed Mr. Mohs's designations for two of the Systems of Crystallization, the *Pyramidal*



and the *Prismatic*, as not characteristic. If it were thought advisable to reform such a defect, this might be done by calling them the *Square Pyramidal* and the *Oblong Prismatic*, which terms, while they expressed the real distinction of the systems, would be intelligible at once to those acquainted with the Mohsian terminology.

I will mention another suggestion respecting the introduction of an improvement in scientific language. The term *Depolarization* was introduced, because it was believed that the effect of certain crystals, when polarized light was incident upon them in certain positions, was to destroy the peculiarity which polarization had produced. But it is now well known that the effect of the second crystal in general is to divide the polarized ray of light into two rays, polarized in different planes. Still this effect is often spoken of as *Depolarization*, no better term having been yet devised. I have proposed and used the term *Dipolarization*, which well expresses what takes place, and so nearly resembles the older word, that it must sound familiar to those already acquainted with writings on this subject.

I may mention one term in another department of literature which it appears desirable to reform in the same manner. The theory of the Fine Arts, or the philosophy which speculates concerning what is beautiful in painting, sculpture or architecture, and other arts, often requires to be spoken of in a single word. Baumgarten and other German writers have termed this province of speculation *Æsthetics*; αἰσθάνεσθαι, to *perceive*, being a word which appeared to them fit to designate the perception of beauty in particular. Since, however, *æsthetics* would naturally denote the Doctrine of Perception in general; since this Doctrine requires a name;

since the term *æsthetics* has actually been applied to it by other German writers (as Kant); and since the essential point in the philosophy now spoken of is that it attends to Beauty;—it appears desirable to change this name. In pursuance of the maxim now before us, I should propose the term *Callæsthetics*, or rather (in agreement with what was said in page 561) *Callæsthetic*, the science of the perception of beauty.

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# **A P P E N D I X.**

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## **PHILOSOPHICAL ESSAYS**

**PREVIOUSLY PUBLISHED.**



# ESSAY I.

## ON THE NATURE OF THE TRUTH OF THE LAWS OF MOTION (1834)\*.

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1. THE long continuance of the disputes and oppositions of opinion which have occurred among theoretical writers concerning the elementary principles of Mechanics, may have made such discussions appear to some persons wearisome and unprofitable. I might, however, not unreasonably plead this very circumstance as an apology for offering a new view of the subject ; since the extent to which these discussions have already gone shews that some men at least take a great interest in them ; and it may be stated, I think, without fear of contradiction, that these controversies have not terminated in the general and undisputed establishment of any one of the antagonist opinions.

The question to which my remarks at present refer is this : “ What is the kind and degree of cogency of the best proofs of the laws of motion, or of the fundamental principles of mechanics, exprest in any other way ? ” Are these laws, philosophically considered, *necessary*, and capable of demonstration by means of self-evident axioms, like the truths of geometry ; or are they *empirical*, and only known to be true by trial and observation, like such general rules as we obtain in natural history ?

It certainly appears, at first sight, very difficult to answer the arguments for either side of this alternative. On the one hand it is said, the laws of motion cannot be necessarily true, for if they were so, the denial of them would involve a contradiction. But this it does not, for we can readily conceive them to be other than they are. We can conceive that a body in motion should have a natural tendency to move slower and slower. And we know that, historically speaking, men did at first suppose the laws of motion to be different from what they are now proved to be. This would have been impossible if the negation of these laws had involved a contradiction of self-evident principles, and consequently had been not only false but

\* From the *Transactions of the Cambridge Philosophical Society*, Vol. v. Part II.

inconceivable. These laws, therefore, cannot be necessary ; and can be duly established in no other way than by a reference to experience.

On the other hand, those who deduce their mechanical principles without any express reference to experiment, may urge, on their side, that, by the confession even of their adversaries, the laws of motion are proved to be true beyond the limits of experience ;—that they are assumed to be true of any new kind of motion when first detected, as well as of those already examined ;—and that it is inexplicable how such truths should be established empirically. They may add that the consequences of these laws are allowed to hold with the most complete and absolute universality ; for instance, the proposition that “ the quantity of motion in the world in a given direction cannot be either increased or diminished,” is conceived to be rigorously exact ; and to have a degree and kind of certainty beyond and above all mere facts of experience ; what other kind of truth than necessary truth this can be, it is difficult to say. And if the conclusions be necessarily true, the principles must be so too.

This apparent contradiction therefore, that a law should be necessarily true and yet the contrary of it conceivable, is what I have now to endeavour to explain ; and this I must do by pointing out what appear to me the true grounds of the laws of motion.

2. The science of Mechanics is concerned about motions as determined by their causes, namely, forces ; the nature and extent of the truth of the first principles of this science must therefore depend upon the way in which we can and do reason concerning *causes*. In what manner we obtain the conception of cause, is a question for the metaphysician, and has been the subject of much discussion. But the general principle which governs our mode of viewing occurrences with reference to this conception, so far as our present subject is concerned, does not appear to be disturbed by any of the arguments which have been adduced in this controversy. This principle I shall state in the form of an axiom, as follows.

AXIOM I.—*Every change is produced by a cause.*

It will probably be allowed that this axiom expresses a universal and constant conviction of the human mind ; and that

in looking at a series of occurrences, whether for theoretical or practical purposes, we inevitably and unconsciously assume the truth of this axiom. If a body at rest moves, or a body in motion stops, or turns to the right or the left, we cannot conceive otherwise than that there is some cause for this change. And so far as we can found our mechanical principles on this axiom, they will rest upon as broad and deep a basis as any truths which can come within the circle of our knowledge.

I shall not attempt to analyse this axiom further. Different persons may, according to their different views of such subjects, call it a law of our nature that we should think thus, or a part of the constitution of the human mind, or a result of our power of seeing the true relations of things. Such variety of opinion or expression would not affect the fundamental and universal character of the conviction which the axiom expresses; and would therefore not interfere with our future reasonings.

3. There is another axiom connected with this, which is also a governing and universal principle in all our reasoning concerning causes. It may be thus stated.

AXIOM II.—*Causes are measured by their effects.*

EVERY effect, that is, every change in external objects, implies a cause, as we have already said: and the existence of the cause is known only by the effects it produces. Hence the intensity or magnitude of the cause cannot be known in any other manner than by these effects: and, therefore, when we have to assign a measure of the cause, we must take it from the effects produced.

In what manner the effects are to be taken into account, so as to measure the cause for any particular purpose, will have to be further considered; but the axiom, as now stated, is absolutely and universally true, and is acted upon in all parts of our knowledge in which causes are measured.

4. But something further is requisite. We not only consider that all changes of motion in a body have a cause, but that this cause may reside in other bodies. Bodies are conceived to act upon one another, and thus to influence each other's motions, as when one billiard ball strikes another. But when this happens, it is also supposed that the body struck influences the motion of the striking body. This is included in our notion of body or matter. If one ball could strike and



affect the motions of any number of others without having its own motion in any degree affected, the struck balls would be considered, not as bodies, but as mere shapes or appearances. Some reciprocal influence, some resistance, in short some *reaction*, is necessarily involved in our conception of action among bodies. All mechanical action upon matter implies a corresponding reaction; and we might describe matter as that which resists or reacts when acted on by force. Not only must there be a reaction in such cases, but this reaction is defined and determined by the action which produces it, and is of the same kind as the action itself. The action which one body exerts upon another is a blow, or a pressure; but it cannot press or strike without receiving a pressure or a blow in return. And the reciprocal pressure or blow depends upon the direct, and is determined altogether and solely by that. But this action being mutual, and of the same kind on each body, the effect on each body will be determined by the effect on the other, according to the same rule; each effect in turn being considered as action and the other as reaction. But this cannot be otherwise than by the equality and opposite direction of the action and reaction. And since this reasoning applies in all cases in which bodies influence each others motions, we have the following axiom which is universally true, and is a fundamental principle with regard to all mechanical relations.

AXIOM III.—*Action is always accompanied by an equal and opposite Reaction.*

5. I now proceed to shew in what manner the Laws of Motion depend upon these three axioms.

Bodies move in lines straight or curved, they move more or less rapidly, and their motions are variously affected by other bodies. This succession of occurrences suggests the conceptions of certain properties or attributes of the motions of bodies, as their direction and velocity, by means of which the laws of such occurrences may be exprest. And these properties or attributes are conceived as belonging to the body at each *point* of its motion, and as changing from one point to another. Thus the body, at each point of its path, moves in a certain direction, and with a certain velocity.

These properties, direction and velocity for instance, are subject to the rule stated in the first axiom: they cannot

change without some cause; and when any changes in the motions of a body are seen to depend on its position relative to another body or to any part of space, such other body, or such other part of space, is said to exert a *force* upon the moving body. Also the force exerted upon the moving body is considered to be of a certain value at each point of the body's motion; and though it may change from one point to another, its changes must depend upon the position of the points only, and not upon the velocity and direction of the moving body. For the force which acts upon the body is conceived as a property of the bodies, or points, or lines, or surfaces among which the moving body is placed; the force at all points therefore depends upon the position with regard to the bodies and spaces of which the force is a property; but remains the same, whatever be the circumstances of the body moved. The circumstances of the body moved cannot be a cause which shall change the force acting at any point of space, although they may alter the *effect* which that force produces upon the body. Thus, gravity is the same force at the same point of space, whether it have to act upon a body at rest or in motion; although it still remains to be seen whether it will produce the same effect in the two cases.

6. This being established, we can now see of what nature the laws of motion must be, and can state in a few words the proofs of them. We shall have a law of motion corresponding to each of the above three axioms; the first law will assert that when no force acts, the properties of the motion will be constant; the second law will assert that when a force acts, its quantity is measured by the effect produced; the third law will assert that, when one body acts upon another, there will be a reaction, equal and opposite to the action. And so far as the laws are announced in this form, they will be of absolute and universal truth, and independent of any particular experiment or observation whatever.

But though these laws of motion are necessarily and infallibly true, they are, in the form in which we have stated them, entirely useless and inapplicable. It is impossible to deduce from them any definite and positive conclusions, without some additional knowledge or assumption. This will be clear by stating, as we can now do in a very small compass, the proofs

of the laws of motion in the form in which they are employed in mechanical reasonings.

7. First, of the first Law;—that *a body not acted upon by any force will go on in a straight line with an invariable velocity.*

The body will go on in a straight line: for, at any point of its motion, it has a certain direction, which direction will, by Axiom I, continue unchanged, except some cause make it deviate to one side or other of its former position. But any cause which should make the direction deviate towards any part of space would be a force, and the body is not acted upon by any force. Therefore, the direction cannot change, and the body will go on in the same straight line from the first.

The body will move with an invariable velocity. For the velocity at any point will, by Axiom I, continue unchanged, except some cause make it increase or decrease. And since, by supposition, the body is not acted upon by any force, there can be no such cause depending upon position, that is, upon relations of *space*; for any cause of change of motion which has a reference to space is force.

Therefore there can be no cause of change of motion, except there be one depending upon *time*, such, for instance, as would exist if bodies had a natural tendency to move slower and slower, according to a rate depending on the time elapsed.

But if such cause existed, its effects ought to be considered separately; and it would still be requisite to assume the permanence of the same velocity, as the first law of motion; and to obtain, in addition to this, the laws of the retardation depending on the time.

Whether there is any such cause of retardation in the actual motions of bodies, can be known only by a reference to experience; and by such reference it appears that there is no such cause of the diminution of velocity depending on time alone; and therefore that the first law of motion may, in all cases in which bodies are exempt from the action of external forces, be applied without any addition or correction depending upon the time elapsed.

It is not here necessary to explain at any length in what manner we obtain from experience the knowledge of the truth

just stated, that there is not in the mere lapse of time any cause of the retardation of moving bodies. The proposition is established by shewing that in all the cases in which such a cause appears to exist, the cause of retardation resides in surrounding bodies and not in time alone, and is therefore an external force. And as this can be shewn in every instance, there remains only the negation of all ground for the assumption of such a cause of retardation. We therefore reject it altogether.

Thus it appears that in proving the first law of motion, we obtain from our conception of cause the conviction that velocity will be uniform except some cause produce a change in it; but that we are compelled to have recourse to experience in order to learn that time alone is not a cause of change of velocity.

8. I now proceed to the second Law:—that *when a force acts upon a body in motion, the effect is the same as that which the same force produces upon a body at rest.*

This law requires some explanation. How is the effect produced upon a moving body to be measured, so that we may compare it with the effect upon a body at rest? The answer to this is, that we here take for the measure of the effect of the force, that motion which must be *compounded* with the motion existing before the change, in order to produce the motion which exists after the change: the rules for the composition of motion being established on independent grounds by the aid of definition alone. Thus if gravity act upon a body which is falling vertically, the effect of gravity upon the body is measured by the velocity *added* to that which the body already has: if gravity act upon a body which is moving horizontally, its effect is measured by the distance to which the body falls below the horizontal line.

The effect of the force which we consider in the second Law of motion, is its effect upon velocity only: and it is proper to mark this restriction by an appropriate term: we shall call this the *accelerative effect* of force; and the cause, as measured by this effect, may be termed the *accelerative quantity* of the force\*.

\* The accelerative quantity of a force (the *quantitas acceleratrix vis cujusvis* of Newton) is often called the *accelerating force*; and we may thus have to speak of the *accelerating force of a certain force*,

A law of motion which necessarily results from our second Axiom is, that the accelerative quantity of a force is measured by the accelerative effect. But whether the accelerative effect depends upon the velocity and direction of the moving body, cannot be known independently of experience. It is very conceivable, for instance, that the force of gravity being every where the same, shall yet produce, upon falling bodies, a smaller accelerative effect in proportion to the velocity which they already have in a downward direction. Indeed if gravity resembled in its operation the effect of any other mode of mechanical agency, the result would be so. If a body moved downwards in consequence of the action of a hand pushing it with a constant effort, or of a spring, or of a stream of fluid rushing in the same direction, the accelerative effect of such agents would be smaller and smaller as the velocity of the body propelled was larger and larger. We can learn from experience alone that the effects of the action of gravity do not follow the same rule.

We assert that the accelerative quantity of the *same force* of gravity is the same whatever be the motion of the body acted on. It may be asked how we know that the force of gravity is the same in cases so compared; for instance, when it acts on a body at rest and in motion? The answer to this question we have given already. By the very process of considering gravity as a force, we consider it as an attribute of something independent of the body acted on. The amount of the force may depend upon place, and even time, for any thing we know *à priori*; but we do not find that the weight of bodies depends on these circumstances, and therefore, having no evidence of a difference in the force of gravity, we suppose it the same at different times and places. And as to the rest, since the force is a force which acts on the body, it is considered as the same force, whatever be the circumstances of the passive body, although the *effects* may vary with these circum-

which is at any rate an awkward phraseology. It would perhaps have been fortunate if Newton, or some other writer of authority, at the time when the principles of mechanics were first clearly developed, had invented an abstract term for this quantity: it might for instance have been called *accelerativity*. And the second law of motion would then have been, that the *accelerativity* of the same force is the same, whatever be the motion of the body acted on. •

stances. If the effects are liable to such change, this change must be considered separately, and its laws investigated; but it cannot be allowed to unsettle our assumption of the permanence of the force itself. It is precisely this assumption of a constant cause, which gives us a fixed term, as a means of estimating and expressing by what conditions the effects are regulated.

It appears by observation and experiment, that the accelerative quantity of the same force is not affected by the velocity or direction of the body acted on: for instance, a body falling vertically receives, in any second of time, an accession of velocity as great as that which it received in the first second, notwithstanding the velocity with which it is already moving. The proof of this and similar assertions from experiment produced, historically speaking, the establishment of the second law of motion in the sense in which we now assert it. And here, as in the case of the first law, we may observe that an important portion of the process of proof consisted in shewing that in those cases in which the *accelerative effect* of a force appeared to be changed by the circumstances of the motion of the body acted on, the change was, in fact, due to other external forces; so that all evidence of a cause of change residing in those circumstances was entirely negatived; and thus the law, that the accelerative effect of the same force is the same, appeared to be absolutely and rigorously true.

9. When the motions of bodies are not effected merely by forces like gravity, which are only perceived by their effects, but are acted upon by other bodies, the case requires other considerations.

It is in such cases that we originally form the conception of force; we ourselves pull and push, thrust and throw bodies, with a view, it may be, either to put them in motion, or to prevent their moving, or to alter their figure. Such operations, and the terms by which they are described, are all included in the term *force*, and in other terms of cognate import. And in using this term, we necessarily assume and imply the co-existence of these various effects of force which we have observed universally to accompany each other. Thus the same kind of force which is the cause of motion, may also be the cause of a body having a form different from its natural form; when we draw a bow, the same kind of pull is needed to move the string,

and to hold it steady, when the bow is bent. And a weight might be hung to the string, so as to produce either the one or the other of these effects. By an infinite multiplicity of experiments of this kind, we become imbued with the conviction that the same pressure may be the cause of tension and of motion. Also as the cause can be known by its effects only, each of these effects may be taken as its measure ; and therefore, so long as one of them is the same, since the cause is the same, the other must be the same also. That is, so long as the pressure or force which shews itself in tension is the same, the motion which it would produce must, under the same circumstances, be the same also. This general fact is not a result of any particular observations, but of the general observation or suggestion arising unavoidably from universal experience, that both tension and motion may be referred to force as their cause, and have no other cause.

We come therefore to this principle with regard to the actions of bodies upon each other, that so long as the tension or pressure is the same, the force, as shewn by its effect in producing motion, must also be the same.

10. This force or action of bodies upon one another, is that which is meant in the Third Axiom, and we now proceed to consider the application of this axiom in mechanics.

Pressures or forces such as I have spoken of, may be employed in producing tension only, and not motion ; in this case, each force prevents the motion which would be produced by the others, and the forces are said to balance each other, or to be in equilibrium. The science which treats of such cases is called Statics, and it depends entirely upon the above third axiom, applied to pressures producing rest. It follows from that axiom, that pressures, which acting in opposite directions thus destroy each other's effects, must be equal, each measuring the other. Thus if a man supports a stone in his hand, the force or effort exerted by the man upwards is equal to the weight or force of the stone downwards. And if a second stone, just equal to the first, were supported at the same time in the same hand, the force or effort must be twice as great ; for the two stones may be considered as one body of twice the magnitude, and of twice the weight ; and therefore the effort which supports it must also be twice as great. And thus we see in what manner statical forces are to be measured in virtue

of this third axiom ; and no further principle is requisite to enable us to establish the whole doctrine of statics.

11. The third axiom, when applied to the actions of bodies in motion, gives rise to the third law of motion, which we must now consider. Here, as in the cases of the other axioms, we must inquire how we are to measure the quantities to which the axiom applies. What is the measure of the *action* which takes place when a body is put in motion by pressure or force? In order to answer this question, we must consider what circumstances make it requisite that the force should be greater or less. If we have to lift a stone, the force which we exert must be greater when the stone is greater: again, we must exert a greater force to lift it quickly than slowly. It is clear, therefore, that that property of a force with which we are here concerned, and which we may call the *motive quantity* of the force\*, increases both when the velocity communicated, and when the mass moved, increase, and depends upon both these quantities, though we have not yet shewn what is the law of this dependence.

The condition that a quantity  $P$  shall increase when each of two others  $V$  and  $M$  does so, may be satisfied in many ways: for instance, by supposing  $P$  proportional to the sum  $M + V$ , (all the quantities being expressed in numbers), or to the product,  $MV$ , or to  $MV^2$ , or in many other ways.

When, however, the quantities  $V$  and  $M$  are altogether heterogeneous, as when one is velocity, and the other weight, the first of the above suppositions, that  $P$  varies as  $M + V$ , is inadmissible. For the law of variation of the formula  $M + V$  depends upon the relation of the units by which  $M$  and  $V$  respectively are measured ; and as these units are arbitrary in each case, the result is, in like manner, arbitrary, and therefore cannot express a law of nature.

12. The supposition that the motive quantity of a force

\* The motive quantity of a force (*vis cujusvis quantitas motrix* of Newton) is sometimes called *moving force*; we are thus led to speak of the moving force of a force, as we have already observed concerning accelerating force. Hence, as in that case, we might employ a single term, as *motivity*, to denote this property of force ; and might thus speak of it and of its measures without the awkwardness which arises from the usual phrase.



varies as  $M + V$ , where  $M$  is the mass moved and  $V$  the velocity, being thus inadmissible, we have to select upon due grounds, among the other formulæ  $MV$ ,  $MV^2$ ,  $M^2V$ , &c.

And in the first place I observe that the formula must be proportional to  $M$  simply (excluding  $M^2$ , &c.) for both the forces which produce motion and the masses in which motion is produced are capable of addition by juxtaposition, and it is easily seen by observation that such addition does not modify the motion of each mass. If a certain pressure upon one brick (as its own weight) cause it to fall with a certain velocity, an equal pressure on another equal brick will cause it also to fall with the same velocity; and these two bricks being placed in contact, may be considered as one mass, which a double force will cause to fall with still the same velocity. And thus all bodies, whatever be their magnitude, will fall with the same velocity by the action of gravity. Those who deny this (as the Aristotelians did) must maintain, that by establishing between two bodies such a contact as makes them one body, we modify the motion which a certain pressure will produce in them. And when we find experimentally (as we do find) that large bodies and small ones fall with the same velocity, excluding the effects of extraneous forces, this result shews that there is not, in the union of small bodies into a larger one, any cause which affects the motion produced in the bodies.

It appears, therefore, that the motive quantity of force which puts a body in motion is, *ceteris paribus*, proportional to the mass of the body; so that for a double mass a double force is requisite, in order that the velocity produced may be the same. Mass considered with reference to this rule, is called *Inertia*.

13. The measure of mass which is used in expressing a law of motion, must be obtained in some way independent of motion, otherwise the law will have no meaning. Therefore, mass measured in order to be considered as *Inertia* must be measured by the statical effects of bodies, for instance, by comparison of weights. Thus two masses are equal which each balance the same weight in the same manner; and a mass is double of one of them which produces the same effect as the two. And we find, by universal observations, that the weight of a mass is not affected by the figure or the arrangement of parts, so long as the matter continues the same. Hence it

appears that the mass of bodies must be compared by comparing their weights, and Inertia is proportional to weight at the same place.

Since all bodies, small or large, light or heavy, fall downwards with equal velocities, when we remove or abstract the effect of extraneous circumstances, the motive quantity of the force of gravity on equal bodies is as their masses; or as their weight, by what has just been said.

14. For the measure of the motive quantity of force, or of the action and reaction of bodies in motion, we have, therefore, now to choose among such expressions as  $MV$ , and  $MV^2$ . And our choice must be regulated by finding what is the measure which will enable us to assert, in all cases of action between bodies in motion, that action and reaction are equal and opposite.

Now the fact is, that either of the above measures may be taken, and each has been taken by a large body of mathematicians. The former however ( $MV$ ) has obtained the designation which naturally falls to the lot of such a measure; and is called *momentum*, or sometimes simply *quantity of motion*: the latter quantity ( $MV^2$ ) is called *vis viva* or *living force*.

I have said that either of these measures may be taken: the former must be the measure of action, if we are to measure it by the effect produced *in a given time*; the latter is the measure if we take the *whole* effect produced. In either way the third law of motion would be true.

Thus if a ball  $B$ , lying on a smooth table, be drawn along by a weight  $A$  hanging by a thread over the edge of the table, the motion of  $B$  is produced by the action of  $A$ , and on the other hand the motion of  $A$  is diminished by the reaction of  $B$ ; and the equality of action and reaction here consists in this, that the momentum ( $MV$ ) which  $B$  acquires in any time is equal to that which  $A$  loses: that is, so much is taken from the momentum which  $A$  would have had, if it had fallen freely *in the same time*; so that  $A$  falls more slowly by just so much.

But if the weight  $A$  fall through a given space from rest, as 1 foot, and then cease to act, the equality of action and reaction consists in this, that the *vis viva* which  $B$  acquires on the whole, is equal to the *vis viva* which  $A$  loses; that is, the *vis viva* of  $A$  thus acting on  $B$  is smaller by so much than it would have been, if  $A$  had fallen freely *through the same space*.

15. In fact, these two propositions are necessarily connected, and one of them may be deduced from the other. The former way of stating the third law of motion appears, however, to be the simplest mode of treating the subject, and we may put the third law of motion in this form.

*In the direct mutual action of bodies, the momentum gained and lost in any time are equal.*

This law depends upon experiment, and is perhaps best proved by some of its consequences. It follows from the law so stated, that the motive quantity of a force is proportional to the momentum generated in a given time; since the motive quantity of force is to be equivalent to that action and reaction which is understood in the third law of motion. Now, if the pressure arising from the weight of a body  $P$  produce motion in a mass  $Q$ , since the momentum gained by  $Q$  and that lost by  $P$  in any time are equal, the momentum of the whole at any time will be the same as if  $P$ 's weight had been employed in moving  $P$  alone. Therefore, the velocity of the mass  $Q$  will be less, in the same proportion in which the mass or inertia is greater: and thus the accelerating quantity of the force is inversely proportioned to the mass moved. This rule enables us to find the accelerative quantity of the force in various cases, as for instance, when bodies oscillate, or when a smaller weight moves a large mass; and we can hence calculate the circumstances of the motion, which are found to agree with the consequences of the above law.

16. But the argument may be reduced to a simpler form. Our object is to shew that, for an equal mass, the velocity produced by a force acting for a given time is as the pressure which produces the motion; for instance, that a double pressure will produce a double velocity. Now a double pressure may be considered as the union of two equal pressures, and if these two act *successively*, the first will communicate to the body a certain velocity, and the second will communicate an additional velocity, equal to the first, by the second law of motion; so that the whole velocity thus communicated will be the double of the first. Therefore, if the velocity communicated be not also the double of the first when the two pressures act *together*, the difference must arise from this, that the effect of one force is modified by the simultaneous action of the other. And when we find by experience (as we do find) that there is no such

difference, but that the velocity communicated in a given time is as the pressure which communicates it, this result shews that there is nothing in the circumstance of a body being already acted on by one pressure, which modifies the effect of an additional pressure acting along with the first.

17. I have above asserted the law, of the *direct* action of bodies only. But it is also true when the action is indirect, as when by turning a winch we move a wheel, the main mass of which is farther from the axis than the handle of the winch. In this case the pressure we exert acts at a mechanical disadvantage on the main mass of the wheel, and we may ask whether this circumstance introduces any new law of motion. And to this we may reply, that we can *conceive* pressure to produce different effects in moving bodies, according as it is exerted directly or by the intervention of machines; but that we *find* no reason to believe that such a difference exists. The relations of the pressures in different parts of a machine are determined by considering the machine at rest. But if we suppose it to be put in motion by such pressures, we see no reason to expect that these pressures should have a different relation to the motions produced from what they would have done if they were direct pressures. And as we find in experiment a negation of all evidence of such a difference, we reject the supposition altogether. We assert, therefore, the third law of motion to be true, whatever be the mechanism by the intervention of which action and reaction are opposed to each other.

From this consideration it is easy to deduce the following rule, which is known by the designation of D'Alembert's principle, and may be considered as a fourth law of motion.

*When any forces produce motion in any connected system of matter, the motive quantities of force gained and lost by the different parts must balance each other according to the connexion of the system.*

By the motive quantity of force *gained* by any body, is here meant the quantity by which that motive force which the body's motion implies (according to the measures already established) exceeds the quantity of motive force which acts immediately upon the body. It is the excess of the *effective* above the *impressed* force, and of course arises from the force transmitted from the other bodies of the system in consequence of the connexion of the parts. The motive quantity of force *lost* is in like

manner the excess of the impressed above the effective force. And these two excesses, in different parts of the system, must balance each other according to the mechanical advantage or disadvantage at which they act for each part.

This completes our system of mechanical principles, and authorizes us to extend to bodies of any size and form the rules which the second law of motion gives for the motion of bodies considered as points. And by thus enabling us to trace what the motions of bodies will be according to the rule asserted in the third law of motion, (namely, that the motive quantity of forces is as the momentum produced in a given time,) it leads us to verify that supposition by experiments in which bodies oscillate or revolve or move in any regular and measurable manner, as has been done by Atwood, Smeaton, and many others.

18. We have thus a complete view of the nature and extent of the fundamental principles of mechanics; and we now see the reason why the laws of motion are so many and no more, in what way they are independent of experience, and in what way they depend upon experiment. The form, and even the language of these laws is of necessity what it is; but the interpretation and application of them is not possible without reference to fact. We may imagine many rules according to which bodies might move (for many sets of rules, different from the existing ones, are, so far as we can see, possible) and we should still have to assert—that velocity could not change without a cause,—that change of action is proportional to the force which produces it,—and that action and reaction are equal and opposite. The truth of these assertions is involved in those notions of causation and matter, which the very attempt to know any thing concerning the relations of matter and motion presupposes. But, according to the facts which we might find, in such imaginary cases as I have spoken of, we should settle in a different way—what is a cause of change of velocity,—what is the measure of the force which changes motion,—and what is the measure of action between bodies. The law is necessary, if there is to be a law; the meaning of its terms is decided by what we find, and is therefore regulated by our special experience.

19. It may further illustrate this matter to point out that this view is confirmed by the history of mathematics. The laws of motion were assented to as soon as propounded; but were

yet each in its turn the subject of strenuous controversy. The terms of the law, the form, which is necessarily true, were recognized and undisputed; but the meaning of the terms, the substance of the law, was loudly contested; and though men often tried to decide the disputed points by pure reasoning, it was easily seen that this could not suffice; and that since it was a case where experience *could* decide, experience *must* be the proper test: since the matter came within her jurisdiction, her authority was single and supreme.

Thus with regard to the first law of motion, Aristotle allowed that *natural* motions continue unchanged, though he asserted the motions of terrestrial bodies to be *constrained* motions, and therefore, liable to diminution. Whether this was the cause of their diminution was a question of fact, which was, by examination of facts, decided against Aristotle. In like manner, in the first case of the second law of motion which came under consideration, both Galileo and his opponent agree that falling bodies are *uniformly* accelerated; that is, that the force of gravity accelerates a body uniformly whatever be the velocity it has already; but the question arises, what is uniform acceleration? It so happened in this case, that the first conjecture of Galileo, afterwards defended by Casræus, (that the velocity was proportional to the space from the beginning of the motion,) was not only contradictory to fact, but involved a self-contradiction; and was, therefore, easily disposed of. But this accident did not supersede the necessity of Galileo and his pupils verifying their assertion by reference to experiment, since there were many suppositions which were different from theirs, and still possible, though that of Casræus was not.

The mistake of Aristotle and his followers, in maintaining that large bodies fall more quickly than small ones, in exact proportion to their weight, arose from perceiving half of the third law of motion, that the velocity increases with the force which produces it; and from overlooking the remaining half, that a greater force is required for the same velocity, according as the mass is larger. The ancients never attained to any conception of the force which moves and the body which is moved, as distinct elements to be considered when we enquire into the subject of motion, and therefore could not even propose to themselves in a clear manner the questions which the third law of motion answered.

But, when, in more modern times, this distinction was brought into view, the progress of opinion in this case was nearly the same as with regard to the other laws.

It was allowed at once, and by all, that action and reaction are equal; but the controversy concerning the sense in which this law is to be interpreted, was one of the longest and fiercest in the history of mathematics, and the din of the war has hardly yet died away. The disputes concerning the measure of the force of bodies in motion, or the *vis viva*, were in fact a dispute which of two measures of action that I have mentioned above should be taken; the effect in a given time, or the whole effect: in the one case the *momentum* ( $MV$ ), in the other the *vis viva* ( $MV^2$ ), was the proper measure.

20. It may be observed that the word *momentum*, which one party appropriated to their views, was employed to designate the motive quantity of force, or the action of bodies in motion, before it was determined what the true measure of such action was. Thus Galileo, in his "Discorso intorno alle cose che stanno in su l'Acqua," says, that momentum "is the force, efficacy, or virtue with which the motion moves and the body moved resists; depending not on weight only, but on the velocity, inclination, and any other cause of such virtue."

The adoption of the phrase *vis viva* is another instance of the extent to which men are tenacious of those terms which carry along with their use a reference to the fundamental laws of our thought on such matters. The party which used this phrase maintained that the mass multiplied into the square of the velocity was the proper measure of the force of bodies in motion; but finding the term *moving force* appropriated by their opponents, they still took the same term *force*, with the peculiar distinction of its being *living* force, in opposition to *dead* force or pressure, which they allowed to be rightly measured by the momentum generated in a given time. The same tendency to adopt, in a limited and technical sense, the words of most general and fundamental use in the subject, has led some writers (Newton for instance,) to employ the term *motion* or *quantity of motion* as synonymous with momentum, or the product of the numbers which express the mass and the velocity. And this use being established, the quantities of motion gained and lost are always equal and opposite; and, therefore the quantity which exists in any given direction can-

not be increased or diminished by any mutual action of bodies. Thus we are led to the assertion which has already been noticed, that the quantity of motion in the world is always the same. And we now see how far the necessary truth of this proposition can be asserted. The proposition is necessarily true according to our notions of material causation; but the measure of "quantity of motion," which is a condition of its truth, is inevitably obtained from experience.

21. It is not surprising that there should have been a good deal of confusion and difference of opinion on these matters: for it appears that there is, in the intellectual constitution and faculties of man, a source of self-delusion in such reasonings. The actual rules of the motion and mutual action of bodies are, and must be, obtained from observation of the external world: but there is a constant wish and propensity to express these rules in such terms as shall make them appear self-evident, because identical with the universal and necessary rules of causation. And this propensity is essential to the progress of our knowledge; and in the success of this effort consists, in a great measure, the advance of the science to its highest point of simplicity and generality.

22. The nature of the truth which belongs to the laws of motion will perhaps appear still more clearly, if we state, in the following tabular form, the analysis of each law into the part which is necessary, and the part which is empirical.

|             | <i>Necessary.</i>                                                              | <i>Empirical.</i>                                                                                                                                                                                                 |
|-------------|--------------------------------------------------------------------------------|-------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------|
| First Law.  | Velocity does not change without a cause.                                      | The time for which a body has already been in motion is not a cause of change of velocity.                                                                                                                        |
| Second Law. | The accelerating quantity of a force is measured by the acceleration produced. | The velocity and direction of the motion which a body already possesses are not, either of them, causes which change the acceleration produced.                                                                   |
| Third Law.  | Reaction is equal and opposite to action.                                      | The connexion of the parts of a body, or of a system of bodies, and the action to which the body or system is already subject, are not, either of them, causes which change the effects of any additional action. |



Of course, it will be understood that, when we assert that the connexion of the parts of a system does not change the effect of any action upon it, we mean that this connexion does not introduce any *new* cause of change, but leaves the effect to be determined by the previously established rules of equilibrium and motion. The connexion will modify the application of such rules; but it introduces no additional rule: and the same observation applies to all the above stated empirical propositions.

This being understood, it will be observed that the part of each law which is here stated as empirical, consists, in each case, of a negation of the supposition that the condition of the moving body with respect to motion and action, is a cause of any change in the circumstances of its motion; and from this it follows that these circumstances are determined entirely by the forces extraneous to the body itself.

23. This mode of considering the question shows us in what manner the laws of motion may be said to be proved by their simplicity, which is sometimes urged as a proof. They undoubtedly have this distinction of the greatest possible simplicity, for they consist in the negation of all causes of change, except those which are essential to our conception of such causation. We may conceive the motions of bodies, and the effect of forces upon them, to be regulated by the lapse of time, by the motion which the bodies have, by the forces previously acting; but though we may imagine this as possible, we do not find that it is so in reality. If it were, we should have to consider the effect of these conditions of the body acted on, and to combine this effect with that of the acting forces; and thus the motion would be determined by more numerous conditions and more complex rules than those which are found to be the laws of nature. The laws which, in reality, govern motion are the fewest and simplest possible, because all are excluded, except those which the very nature of laws of motion necessarily implies. The prerogative of simplicity is possessed by the actual laws of the universe, in the highest perfection which is imaginable or possible. Instead of having to take into account all the circumstances of the moving bodies, we find that we have only to reject all these circumstances. Instead of having to combine empirical with necessary laws, we learn empirically that the necessary laws are entirely sufficient.

24. Since all that we learn from experience is, that she

has nothing to teach us concerning the laws of motion, it is very natural that some persons should imagine that experience is not necessary to their proof. And accordingly many writers have undertaken to establish all the fundamental principles of mechanics by reasoning alone. This has been done in two ways:—sometimes by attending only to the necessary part of each law (as the parts are stated in the last paragraph but one) and by overlooking the necessity of the empirical supplement and limitation to it;—at other times by asserting the part which I have stated as empirical to be self-evident, no less than the other part. The former way of proceeding may be found in many English writers on the subject; the latter appears to direct the reasonings of many eminent French mathematicians. Some (as Laplace) have allowed the empirical nature of two out of the three laws; others, as M. Poisson, have considered the first as alone empirical; and others, as D'Alembert, have assumed the self-evidence of all the three independently of any reference whatever to observation.

25. The parts of the laws which I have stated as empirical, appear to me to be clearly of a different nature, as to the cogency of their truth, from the parts which are necessary; and this difference is, I think, established by the fact that these propositions were denied, contested, and modified, before they were finally established. If these truths could not be denied without a self-contradiction, it is difficult to understand how they could be (as they were) long and obstinately controverted by mathematicians and others fully sensible to the cogency of necessary truth.

I will not however go so far as to assert that there may not be some point of view in which that which I have called the empirical part of these laws, (which, as we have seen, contains negatives only,) may be properly said to be self-evident. But however this may be, I think it can hardly be denied that there is a difference of a fundamental kind in the nature of these truths,—which we can, in our imagination at least, contradict and replace by others, and which, historically speaking, have been established by experiment;—and those other truths, which have been assented to from the first, and by all, and which we cannot deny without a contradiction in terms, or reject without putting an end to all use of our reason on this subject.

26. On the other hand, if any one should be disposed to maintain that, inasmuch as the laws are interpreted by the aid of experience only, they must be considered as entirely empirical laws, I should not assert this to be placing the science of mechanics on a wrong basis. But at the same time I would observe, that the form of these laws is not empirical, and would be the same if the results of experience should differ from the actual results. The laws may be considered as a formula derived from *à priori* reasonings, where experience assigns the value of the terms which enter into the formula.

Finally, it may be observed, that if any one can convince himself that matter is either necessarily and by its own nature determined to move slower and slower, or necessarily and by its own nature determined to move uniformly, he must adopt the latter opinion, not only of the truth, but of the necessity of the truth of the first law of motion, since the former branch of the alternative is certainly false: and similar assertions may be made with regard to the other laws of motion.

27. This inquiry into the nature of the laws of motion, will, I hope, possess some interest for those who attach any importance to the logic and philosophy of science. The discussion may be said to be rather metaphysical than mechanical; but the views which I have endeavoured to present, appear to explain the occurrence and result of the principal controversies which the history of this science exhibits; and, if they are well founded, ought to govern the way in which the principles of the science are treated of, whether the treatise be intended for the mathematical student or the philosopher.

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## ESSAY II.

### REMARKS ON MATHEMATICAL REASONING AND ON THE LOGIC OF INDUCTION\*.

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#### SECT. I.—*On the Grounds of Mathematical Reasoning.*

1. THE study of a science, treated according to a rigorous system of mathematical reasoning, is useful, not only on account of the positive knowledge which may be acquired on the subjects which belong to the science, but also on account of the collateral effects and general bearings of such a study, as a discipline of the mind and an illustration of philosophical principles.

Considering the study of the mathematical sciences with reference to these latter objects, we may note two ways in which it may promote them ;—by habituating the mind to strict reasoning,—and by affording an occasion of contemplating some of the most important mental processes and some of the most distinct forms of truth. Thus mathematical studies may be useful in teaching practical logic and theoretical metaphysics. We shall make a few remarks on each of these topics.

2. The study of Mathematics teaches strict reasoning—by bringing under the student's notice prominent and clear examples of trains of demonstration :—by exercising him in the habits of attentive and connected thought which are requisite in order to follow these trains ;—and by familiarizing him with the peculiar and distinctive conviction which demonstration produces, and with the rigorous exclusion of all considerations which do not enter into the demonstration.

3. Logic is a system of doctrine which lays down rules for determining in what cases pretended reasonings are and are not demonstrative. And accordingly, the teaching of strict reasoning by means of the study of logic is often recommended and practised. But in order to show the superiority of the study of mathematics for this purpose, we may consider,—that reasoning, as a practical process, must be learnt by practice, in the same manner as any other practical art, for example, riding,

\* From the *Mechanical Euclid*, 1837.

or fencing ;—that we are not secured from committing fallacies by such a classification of fallacies as logic supplies, as a rider would not be secured from falls by a classification of them ;—and that the habit of attending to our mental processes while we are reasoning, rather interferes with than assists our reasoning well, as the horseman would ride worse rather than better, if he were to fix his attention upon his muscles when he is using them.

4. To this it may be added, that the peculiar habits which enable any one to follow a *chain* of reasoning are excellently taught by mathematical study, and are hardly at all taught by logic. These habits consist in not only apprehending distinctly the demonstration of a proposition when it is proved, but in retaining all the propositions thus proved, and using them in the ulterior steps of the argument with the same clear conviction, readiness, and familiarity, as if they were self-evident principles. Writers on Logic seldom give examples of reasoning in which several syllogisms follow each other ; and they never give examples in which this progressive reasoning is so exemplified as to make the process familiar. Their chains generally consist only of two or three links. In mathematics, on the contrary, every theorem is an example of such a chain : every proof consists of a series of assertions, of which each depends on the preceding, but of which the last inferences are no less evident or less easily applied than the simplest first principles. The language contains a constant succession of short and rapid references to what has been proved already ; and it is justly assumed that each of these brief movements helps the reasoner forwards in a course of infallible certainty and security. Each of these hasty glances must possess the clearness of intuitive evidence, and the certainty of mature reflection ; and yet must leave the reasoner's mind entirely free to turn instantly to the next point of his progress. The faculty of performing such mental processes well and readily is of great value, and is in no way fostered by the study of logic.

5. It is sometimes objected to the study of Mathematics as a discipline of reasoning, that it tends to render men insensible to all reasoning which is not mathematical, and leads them to demand, in other subjects, proofs such as the subject does not admit of, or such as are not appropriate to the matter.

To this it may be replied, that these evil results, so far as

they occur, arise either from the student pursuing too exclusively one particular line of mathematical study, or from erroneous notions of the nature of demonstration.

The present volume is intended to assist, in some measure, in remedying the too exclusive pursuit of one particular line of Mathematics, by shewing that the same simplicity and evidence which are seen in the Elements of Geometry may be introduced into the treatment of another subject of a kind very different ; and it is hoped that we may thus bring the subject within the reach of those who cultivate the study of Mathematics as a discipline only. The remarks now offered to the reader are intended to aid him in forming a just judgment of the analogy between mathematical and other proof ; which is to be done by pointing out the true grounds of the evidence of Geometry, and by exhibiting the views which are suggested by the extension of mathematical reasoning to sciences concerned about physical facts.

6. We shall therefore now proceed to make some remarks on the nature and principles of reasoning, especially as far as they are illustrated by the mathematical sciences.

Some of the leading principles which bear upon this subject are brought into view by the consideration of the question, "What is the foundation of the certainty arising from mathematical demonstration?" and in this question it is implied that mathematical demonstration is recognized as a kind of reasoning, possessing a peculiar character and evidence, which make it a definite and instructive subject of consideration.

7. Perhaps the most obvious answer to the question respecting the conclusiveness of mathematical demonstration is this ;—that the certainty of such demonstration arises from its being founded upon *Axioms* ; and conducted by steps, of which each might, if required, be stated as a rigorous *Syllogism*.

This answer might give rise to the further questions, What is the foundation of the conclusiveness of a Syllogism? and, What is the foundation of the certainty of an Axiom? And if we suppose the former inquiry to be left to Logic, as being the subject of that science, the latter question still remains to be considered. We may also remark upon this answer, that mathematical demonstration appears to depend upon Definitions, at least as much as upon Axioms. And thus we are led to these questions :—Whether mathematical demonstration is

founded upon Definitions, or upon Axioms, or upon both? and, What is the real nature of Definitions and of Axioms?

8. The question, What is the foundation of mathematical demonstration? was discussed at considerable length by Dugald Stewart\*; and the opinion at which he arrived was, that the certainty of mathematical reasoning arises from its depending upon *definitions*. He expresses this further, by declaring that mathematical truth is hypothetical, and must be understood as asserting only, that *if* the definitions are assumed, the conclusion follows. The same opinion has, I think, prevailed widely among other modern speculators on the same subject, especially among mathematicians themselves.

9. In opposition to this opinion, I urge, in the first place that no one has yet been able to construct a system of mathematical truth by means of definitions alone, to the exclusion of axioms; although attempts having this tendency have been made constantly and earnestly. It is, for instance, well known to most readers, that many mathematicians have endeavoured to get rid of Euclid's "Axioms" respecting straight lines and parallel lines; but that none of these essays has been generally considered satisfactory. If these axioms could be superseded, by definition or otherwise, it was conceived that the whole structure of Elementary Geometry would rest merely upon definitions; and it was held by those who made such essays, that this would render the science more pure, simple, and homogeneous. If these attempts had succeeded, Stewart's doctrine might have required a further consideration; but it appears strange to assert that Geometry is supported by definitions, and not by axioms, when she cannot stir four steps without resting her foot upon an axiom.

10. But let us consider further the nature of these attempts to supersede the axioms above mentioned. They have usually consisted in endeavours so to frame the definitions, that these might hold the place which the axioms hold in Euclid's reasoning. Thus the axiom, that "two straight lines cannot enclose a space," would be superfluous, if we were to take the following definition:—"A line is said to be *straight*, when two such lines cannot coincide in *two* points without coinciding *altogether*."

But when such a method of treating the subject is proposed,

\* *Elements of the Philosophy of the Human Mind*, Vol. II.

we are unavoidably led to ask,—whether it is allowable to lay down such a definition. It cannot be maintained that we may propound any form of words whatever as a definition, without any consideration whether or not it suggests to the mind any intelligible or possible conception. What would be said, for instance, if we were to state the following as a definition, “A line is said to be *straight* (or any other term) when two such lines cannot coincide in *one* point without coinciding altogether?” It would inevitably be remarked, that no such lines exist; or that such a property of lines cannot hold good without other conditions than those which this definition expresses; or, more generally, that the definition does not correspond to any conception which we can call up in our minds, and therefore can be of no use in our reasonings. And thus it would appear, that a definition, to be admissible, must necessarily refer to and agree with some conception which we can distinctly frame in our thoughts.

11. This is obvious, also, by considering that the definition of a straight line could not be of any use, except we were entitled to apply it in the cases to which our geometrical propositions refer. No definitions of straight lines could be employed in Geometry, unless it were in some way certain that the lines so defined are those by which angles are contained, those by which triangles are bounded, those of which parallelism may be predicated, and the like.

12. The same necessity for some general conception of such lines accompanying the definition, is implied in the terms of the definition above suggested. For what is there meant by “*such* lines?” Apparently, lines having some general character in which the property is necessarily involved. But how does it appear that lines may have such a character? And if it be self-evident that there may be such lines, this evidence is a necessary condition of this (or any equivalent) definition. And since this self-evident truth is the ground on which the course of reasoning must proceed, the simple and obvious method is, to state the property *as* a self-evident truth; that is, as an axiom. Similar remarks would apply to the other axiom above mentioned; and to any others which could be proposed on any subject of rigorous demonstration.

13. If it be conceded that such a conception accompanying the definition is necessary to justify it, we shall have made a



step in our investigation of the grounds of mathematical evidence. But such an admission does not appear to be commonly contemplated by those who maintain that the conclusiveness of mathematical proof results from its depending on definitions. They generally appear to understand their tenet as if it implied *arbitrary* definitions. And something like this seems to be held by Stewart, when he says that mathematical truths are true *hypothetically*. For we understand by an hypothesis a supposition, not only which we may make, but may abstain from making, or may replace by a different supposition.

14. That the fundamental conceptions of Geometry are not arbitrary definitions, or selected hypotheses, will, I think, be clear to any one who reasons geometrically at all. It is impossible to follow the steps of any single proposition of Geometry without conceiving a straight line and its properties, whether or not such a line be defined, and whether or not its properties be stated. That a straight line should be distinguished from all other lines, and that the axiom respecting it should be seen to be true, are circumstances indispensable to any clear thought on the subject of lines. Nor would it be possible to frame any coherent scheme of Geometry in which straight lines should be excluded, or their properties changed. Any one who should make the attempt, would betray, in his first propositions, to all men who can reason geometrically, a reference to straight lines.

15. If, therefore, we say that Geometry depends on definitions, we must add, that they are *necessary*, not arbitrary definitions,—such definitions as we must have in our minds, so far as we have elements of reasoning at all. And the elementary hypotheses of Geometry, if they are to be so termed, are not hypotheses which are requisite to enable us to reach this or that conclusion; but hypotheses which are requisite for *any* exercise of our thoughts on such subjects.

16. Before I notice the bearing of this remark on the question of the necessity of axioms, I may observe that Stewart's disposition to consider definitions, and not axioms, as the true foundation of Geometry, appears to have resulted, in part, from an arbitrary selection of certain axioms, as specimens of all. He takes, as his examples, the axioms, "that if equals be added to equals the wholes are equal," that "the whole is greater than its part;" and the like. If he had, instead of these, considered

the more properly geometrical axioms,—such as those which I have mentioned; “that two straight lines cannot enclose a space;” or any of the axioms which have been made the basis of the doctrine of parallels; for instance, Playfair’s axiom, “that two straight lines which intersect each other cannot both of them be parallel to a third straight line;”—it would have been impossible for him to have considered axioms as holding a different place from definitions in geometrical reasoning. For the properties of triangles are proved from the axiom respecting straight lines, as distinctly and directly, as the properties of angles are proved from the definition of a right angle. Of the many attempts made to prove the doctrine of parallels, almost all professedly, all really, assume some axiom or axioms which are the basis of the reasoning.

17. It is therefore very surprizing that Stewart should so exclusively have fixed his attention upon the more general axioms, as to assert, following Locke, “that from [mathematical] axioms it is not possible for human ingenuity to draw a single inference\*,” and even to make this the ground of a contrast between geometrical axioms and definitions. The slightest examination of any treatise of Geometry might have shown him that there is no sense in which this can be asserted of axioms, in which it is not equally true of definitions; or rather, that while Euclid’s definition of a straight line leads to no truth whatever, his axiom respecting straight lines is the foundation of the whole of Geometry; and that, though we can draw some inferences from the definition of parallel straight lines, we strive in vain to complete the geometrical doctrine of such lines, without assuming some axiom which enables us to prove the converse of our first propositions. Thus, that which Stewart proposes as the distinctive character of axioms, fails altogether; and with it, as I conceive, the whole of his doctrine respecting mathematical evidence.

18. That Geometry (and other sciences when treated in a method equally rigorous) depends upon axioms as well as definitions, is supposed by the form in which it is commonly presented. And after what we have said, we shall assume this form to be a just representation of the real foundations of such sciences, till we can find a tenable distinction between axioms and definitions, in their nature, and in their use; and till we

\* *Elements of the Philosophy of the Human Mind*, Vol. II. p. 38.

have before us a satisfactory system of Geometry without axioms. And this system, we may remark, ought to include the Higher as well as the Elementary Geometry, before it can be held to prove that axioms are needless; for it will hardly be maintained, that the properties of circles depend upon definitions and hypotheses only, while those of ellipses require some additional foundation; or that the comparison of curve lines requires axioms, while the relations of straight lines are independent of such principles.

19. Having then, I trust, cleared away the assertion, that mathematical reasoning rests ultimately upon definitions only, and that this is the ground of its peculiar cogency, I have to examine the real evidence of the truth of such axioms as are employed in the exact Mathematical Sciences. And we are, I think, already brought within view of the answer to this question. For if the definitions of Mathematics are not arbitrary, but necessary, and must, in order to be applicable in reasoning, be accompanied by a conception of the mind through which this necessity is seen; it is clear, that this apprehension of the necessity of the properties which we contemplate, is really the ground of our reasonings and the source of their irresistible evidence. And where we clearly apprehend such necessary relations, it can make no difference whatever in the nature of our reasoning, whether we express them by means of definitions or of axioms. We define a straight line vaguely;—that it is that line which lies evenly between two points: but we forthwith remedy this vagueness, by the axiom respecting straight lines: and thus we express our conception of a straight line, so far as is necessary for reasoning upon it. We might, in like manner, begin by defining a right angle to be the angle made by a line which stands evenly between the two portions of another line; and we might add an axiom, that all right angles are equal. Instead of this, we define a right angle to be that which a line makes with another when the two angles on the two sides of it are equal. But in all these cases, we express our conception of a necessary relation of lines; and whether this be done in the form of definitions or axioms, is a matter of no importance.

20. But it may be asked, If it be thus unimportant whether we state our fundamental principles as axioms or definitions, why not reduce them all to definitions, and thus give to

our system that aspect of independence which many would admire, and with which none need be displeased? And to this we answer, that if such a mode of treating the subject were attempted, our definitions would be so complex, and so obviously dependent on something not expressed, that they would be admired by none. We should have to put into each definition, as conditions, all the axioms which refer to the things defined. For instance, who would think it a gain to escape the difficulties of the doctrine of parallels by such a definition as this: "Parallel straight lines are those which being produced indefinitely both ways do not meet; and which are such that if a straight line intersects one of them it must somewhere meet the other?" And in other cases, the accumulation of necessary properties would be still more cumbersome and more manifestly heterogeneous.

21. The reason of this difficulty is, that our fundamental conception of lines and other relations of space, are capable of being contemplated under several various aspects, and more than one of these aspects are needed in our reasonings. We may take one such aspect of the conception for a definition; and then we must introduce the others by means of axioms. We may define parallels by their not meeting; but we must have some positive property, besides this negative one, in order to complete our reasonings respecting such lines. We have, in fact, our choice of several such self-evident properties, any of which we may employ for our purpose, as geometers well know; but with our naked definition, as they also know, we cannot proceed to the end. And in other cases, in like manner, our fundamental conception gives rise to various elementary truths, the connexion of which is the basis of our reasonings: but this connexion resides in our thoughts, and cannot be made to follow, as a logical result, from any assumed form of words, presented as a definition.

22. If it be further demanded, What is the nature of this bond in our thoughts by which various properties of lines are connected? perhaps the simplest answer is to say, that it resides in *the idea of space*. We cannot conceive things in space without being led to consider them as determined and related in some way or other to straight lines, right angles, and the like; and we cannot contemplate these determinations and relations distinctly, without assuming those properties of straight lines,

of right angles, and of the rest, which are the basis of our Geometry. We cannot conceive or perceive objects at all, except as existing in space; we cannot contemplate them geometrically, without conceiving them in space which is subjected to geometrical conditions; and this mode of contemplation is, by language, analysed into definitions, axioms, or both.

23. The truths thus seen and known, may be said to be known by *intuition*. In English writers this term has, of late, been vaguely used, to express all convictions which are arrived at without conscious reasoning, whether referring to relations among our perceptions, or to conceptions of the most derivative and complex nature. But if we were allowed to restrict the use of this term, we might conveniently confine it to those cases in which we necessarily apprehend relations of things truly, as soon as we conceive the objects distinctly. In this sense axioms may be said to be known by *intuition*; but this phraseology is not essential to our purpose.

24. It appears, then, that the evidence of the axioms of Geometry depends upon a distinct possession of the idea of space. These axioms are stated in the beginning of our Treatises, not as something which the reader is to learn, but as something which he already knows. No proof is offered of them; for they are the beginning, not the end of demonstration. The student's clear apprehension of the truth of these, is a condition of the possibility of his pursuing the reasonings on which he is invited to enter\*. Without this mental capacity,

\* In this statement respecting the nature of Axioms, I find myself agreeing with the acute author of "*Sematology*." See the "*Sequel to Sematology*," p. 103. "An Axiom does not account for an intellection; it does but describe the requisite competency for it." It appears to me that this view is not familiar among English metaphysicians. I may here quote what I said at a former period, "However we may *define* force, it is necessary in order to understand the elementary reasonings of this portion of science, that we should *conceive* it distinctly. Do we wish for a test of the distinctness of our conceptions? The test is, our being able to see the necessary truth of the Axioms on which our reasonings rest...These principles (the Axioms of Statics) are all perfectly evident as soon as we have formed the general conception of pressure; but without that act of thought, they can have no evidence whatever given them by any form of words, or reference to other truths;—by definitions, or by illustrations from other kinds of quantity."—*Thoughts on the Study of Mathematics*, p. 25.

and the power of referring to it, in the reader, the writer's assertions and arguments are empty and unmeaning words; but then, this capacity and power are what all rational creatures alike possess, though habit may have developed it in very various degrees in different persons.

25. It has been common in the school of metaphysicians of which I have spoken, to describe some of the elementary convictions of our minds as *fundamental laws* of belief; and it appears to have been considered that this might be taken as a final and sufficient account of such convictions. I do not know whether any persons would be tempted to apply this formula, as a solution of our question respecting the nature of axioms. If this were proposed, I should observe, that this form of expression seems to me, in such a case, highly unsatisfactory. For *laws* require and enjoin a conjunction of things which can be contemplated separately, and which would be disjoined if the law did not exist. It is a law of nature that terrestrial bodies, when free, fall downwards; for we can easily conceive such bodies divested of such a property. But we cannot say, in the same sense, that the impossibility of two straight lines inclosing a space arises from a law; for if they are straight lines, they need no law to compel this result. We cannot conceive straight lines exempt from such a law. To speak of this property as imposed by a law, is to convey an inadequate and erroneous notion of the close necessity, inviolable even in thought, by which the truth clings to the conception of the lines.

26. This expression, of "laws of belief," appears to have found favour, on this account among others, that it recognized a kind of analogy between the grounds of our reasoning on very abstract subjects, and the principles to which we have recourse in other cases when we manifestly derive our fundamental truths from facts, and when it is supposed to be the ultimate and satisfactory account of them to say, that they are laws of nature learnt by observation. But such an analogy can hardly be considered as a real recommendation by the metaphysician; since it consists in taking a case in which our knowledge is obviously imperfect and its grounds obscure, and in erecting this case into an authority which shall direct the process and control the enquiry of a much more profound and penetrating kind of speculation. It cannot be doubted that we are likely to see the true grounds and evidence of our doctrines much more clearly in the

case of Geometry and other rigorous systems of reasoning, than in collections of mere empirical knowledge, or of what is supposed to be such. It is both an unphilosophical and an indolent proceeding, to take the latter cases as a standard for the former.

27. I shall therefore consider it as established, that in Geometry our reasoning depends upon axioms as well as definitions,—that the evidence of the truth of the axioms and of the propriety of the definitions resides in the idea of space,—and that the distinct possession of this idea, and the consequent apprehension of the truth of the axioms which are its various aspects, is supposed in the student who is to pursue the path of geometrical reasoning. This being understood, I have little further to observe on the subject of Geometry. I will only remark—that all the conclusions which occur in the science follow purely from those first principles of which we have spoken;—that each proposition is rigorously proved from those which have been proved previously from such principles;—that this process of successive proof is termed *Deduction*;—and that the rules which secure the rigorous conclusiveness of each step are the rules of *Logic*, which I need not here dwell upon.

28. But I now proceed to consider some other questions to which our examination of the evidence of Geometry was intended to be preparatory;—How far do the statements hitherto made apply to other sciences? for instance, to such sciences as are treated of in the present volume, Mechanics and Hydrostatics? To this I reply, that some such sciences at least, as for example Statics, appear to me to rest on foundations exactly similar to Geometry:—that is to say, that they depend upon axioms,—self-evident principles, not derived in any immediate manner from experiment, but involved in the very nature of the conceptions which we must possess, in order to reason upon such subjects at all. The proof of this doctrine must consist of several steps, which I shall take in order.

29. In the first place, I say that the axioms of Statics are *self-evidently true*. In the beginning of the Treatise I have stated these barely as axioms, without addition or explanation, as the axioms of Geometry are stated in treatises on that subject. And such is the proper and orderly mode of exhibiting axioms; for, as has been said, they are to be understood as an expression of the condition of conception of the student. They are not to be learnt from without, but from

within. They necessarily and immediately flow from the distinct possession of that idea, which if the student do not possess distinctly, all conclusive reasoning on the subject under notice is impossible. It is not the business of the deductive reasoner to communicate the apprehension of these truths, but to deduce others from them.

30. But though it may not be the author's business to elucidate the truth of the axioms as a deductive reasoner, it may still be desirable that he should do so as a philosophical teacher; and though it may not be possible to add anything to their evidence in the mind of him who possesses distinctly the idea from which they flow, it may be in our power to assist the beginner in obtaining distinct possession of this idea and unfolding it into its consequences. I shall therefore make a few remarks, tending to illustrate the self-evident nature of the "Axioms" of Statics, of Hydrostatics, and of the Doctrine of Motion.

31. Omitting, for the present, the consideration of the First Axiom of Statics (see paragraph 36); the Second is, "If two equal forces act perpendicularly at the extremities of equal arms of a straight line to turn it opposite ways, they will keep each other in equilibrium." This is often, and properly, further confirmed, by observing that there is no reason why one of the forces should preponderate rather than the other, and that, as both cannot preponderate, neither will do so. All the circumstances on which the result (equilibrium or preponderance) can depend, are equal on the two sides;—equal arms, equal angles, equal forces. If the forces are not in equilibrium, *which* will preponderate? no answer can be given, because there is no circumstance left by which either can be distinguished.

32. The argument which we have just used, is often applicable, and may be expressed by the formula, "there is no reason why one of the two opposite cases should occur, which is not equally valid for the other; and as both cannot occur (for they are opposite cases) neither will occur." This argument is called "the principle of sufficient reason;" it puts in a general form the considerations on which several of our axioms depend; and to persons who are accustomed to such generality, it may make their truth more clear.

The same principle might be applied to other cases, for example, to Axiom 7, that the effect produced on a bent lever



does not depend on the direction of the arm. For if we suppose two forces acting perpendicularly on two equal arms of a bent lever to turn it opposite ways, these forces will balance, whatever be the angle which they make, since there is no reason why either should preponderate: but it would thus appear, that the force which would be balanced by  $Q$  in the figure to Axiom 7, would also be balanced by  $R$ , and therefore these two forces produce the same effect; which is what the axiom asserts.

33. The same reasoning might be applied to Axiom 9; for if two equal forces act at right angles at equal arms, in planes perpendicular to the axis of a rigid body, and tend to turn it opposite ways, they will balance each other, since all the conditions are the same for both.

34. Nearly the same might be said of Axiom 10;—if a string pass freely round a fixed body, equal forces acting at its two ends will balance each other; for if it pass with perfect freedom, its passing round the point cannot give an advantage to either force. Therefore the force which will be balanced by the string at its second extremity is exactly equal to the force which acts at its first extremity\*.

35. The axioms which are perhaps least obvious are Axioms 4 and 5; for instance, the former;—that “the pressure upon the fulcrum is equal to the sum of the weights.” Yet this becomes evident when we consider it steadily. It will then be seen that we consider pressure or weight as something which must be supported, so that the whole support must be equal to the whole pressure. The two weights which act upon the lever must be somehow balanced and counteracted, and the length of the lever cannot at all remove or alter this necessity. Their pressure will be the same as if the two arms of the lever were shortened till the weights coincided at the fulcrum; but in this case, it is clear that the pressure on the fulcrum would be equal to the sum of the weights: therefore it will be so in every other case.

36. This principle, that in statical equilibrium, a force is necessarily supported by an equal force, is expressed in Axiom 1, with regard to forces acting at any point; and the two forces are then called action and re-action. The principle, as stated in Axiom 1, may be considered as an expression of the conception of equality as applied to forces, or, if any one chooses, as a

\* The same principle may be applied to prove Ax. 6.

definition of equal forces. This principle is implied in the conception of any comparison of forces; for equilibrium and addition of forces are modes in which forces are compared, as superposition and addition of spaces are modes in which geometrical quantities are compared.

We may further observe, that this fundamental conception of action and reaction is equivalent to the conception of force and matter, which are ideas necessarily connected and correlative. Matter is that which can resist the action of force. In Mechanics at least, we know matter only as the subject on which force acts.

37. But matter not only receives, it also transmits the action of force; and it is impossible to reason respecting the mechanical results of such transmission, without laying down the fundamental principles by which it operates. And this accordingly is the purpose of Axioms 7, 8, 9, 10, 13 [of the *Mechanical Euclid*]. When the body is supposed to be perfectly rigid, it transmits force without any change or yielding. This rigidity of a body is contemplated under different aspects, in the Axioms just referred to. In Axiom 8, it is the rigidity of a rod pushed endways; in Axiom 7, the rigidity of a plane turned about a fixed point; in Axiom 9, the rigidity of a solid twisted about an axis. Axiom 10 defines the manner in which a flexible string transmits pressure, and in like manner Axiom 1 of the Hydrostatics, defines the manner in which a fluid transmits pressure. Any one who chooses may call Axioms 7, 8, 9 of the Statics, collectively, the *Definition* of a rigid body. The place of these principles in our reasoning will not be thereby altered; nor the necessity superseded, of their being accompanied by distinct mechanical conceptions.

38. Axioms 14, 15, 16, of the Statics, are all included in the general consideration that material bodies may be supposed to consist of material parts, and that the weight of the whole is equal to the weight of all the parts; but they are stated separately, because they are used separately, and because they are at least as evident in these more particular cases as they are in the more general form.

By considerations of this nature it appears, and I trust quite satisfactorily, that the axioms, as above stated, are evident in their nature, in virtue of the conceptions which we necessarily form, in order to reason upon mechanical subjects.

39. Some persons may be surprized to find the Axioms of Mechanics represented as so numerous; especially if they look for analogy to Geometry, where the necessary axioms are confessedly few, and according to some writers, none; and they may be led to think that many of the axioms here given must be superfluous, by observing that in most mechanical works the fundamental principles are stated as much fewer than these. But I believe that very few of those which I have stated are superfluous *in effect*. From the very circumstance that they *are* axioms, they are assented to when they are adduced in the reasoning, whether they have been before asserted or not; but to make our reasoning formally correct (which was one of my objects) every proposition which is assumed should be previously stated. And when we examine them, we see that the various modifications and combinations of the ideas of force, body, and equilibrium, along with the ideas of space of one, two, or three dimensions, readily branch out into as many heads as appear in this part of the present work.

40. Some persons may be disposed at first to say, that our knowledge of such elementary truths as are stated in the Axioms of Statics and Hydrostatics, is collected *from observation and experience*. But in refutation of this I remark, that we cannot experimentally verify these elementary truths, without assuming other principles which require proof as much as these do. If, for instance, Archimedes had wished to ascertain by trial whether two equal weights at the equal arms of a lever would balance each other, how could he know that the weights *were* equal, by any more simple criterion than that they *did* balance? But in fact, it is perfectly certain that of the thousands of persons who from the time of Archimedes to the present day have studied Statics as a mathematical science, a very few have received or required any confirmation of his axioms from experiment; and those who have needed such help have undoubtedly been those in whom the apprehension of the real nature and force of the evidence of the subject was most obscure.

41. I by no means intend to assert that the axioms as stated in this Treatise are given in the only exact form; or that they may not be improved, simplified, and reduced in number. But I do not think it likely that this can be done to any great extent, consistently with the rigour of deductive proof. The Fourth Axiom of Statics is one which attempts have been

made to supersede: for example, Lagrange\* has endeavoured to deduce it from the preceding ones. But it will be found that his proof, if distinctly stated, involves some such axiom as this:—that “If two forces, acting at the extremities of a straight line, and a single force, acting at an intermediate point of the straight line, produce the same effect to turn a body about another line, the two forces produce at the intermediate point an effect equal to the single force.” And though this axiom may be self-evident, it will hardly be considered as more simple than that which it replaces.

42. Thus, Statics, like Geometry, rests upon axioms which are neither derived directly from experience, nor capable of being superseded by definitions, nor by simpler principles. In this science, as in that previously considered, the evidence of these fundamental truths resides in those convictions, to which an attentive and steady consideration of the subject necessarily leads us. The axioms with regard to pressures, action and reaction, equilibrium and preponderance, rigid and flexible bodies, result necessarily from the conceptions which are involved in all exact reasoning on such matters. The axioms do not flow *from* the definitions, but they flow irresistibly *along with* the definitions, from the distinctness of our ideas upon the subjects thus brought into view. These axioms are not arbitrary assumptions, nor selected hypotheses; but truths which we must see to be necessarily and universally true, before we can reason on to anything else; and here, as in Geometry, the capacity of seeing that they are thus true, is required in the student, in order that he and the writer may be able to proceed together.

43. It was stated that the Axioms of Geometry, are derived from the idea of space; in like manner the Axioms of Statics are derived from the *idea of statical force* or *pressure*, and the *idea of body* or *matter*, which, as we have said, is correlative with the idea of force. We must possess distinctly this idea of force acting upon body and body sustaining force;—of body resisting, and while it resists, transmitting the action of force;—of body, with this mechanical property, in the various forms of straight line, lever, plane, solid, flexible line, flexible surface, and fluid; and if we possess distinctly the ideas thus pointed out, the truth of the Axioms of Statics and Hydrostatics will be seen as self-evident, and we shall be in a condition to

\* *Mécanique Analytique*. Introduction.

go on with the reasonings [of the *Mechanical Euclid*], seeing both the cogency of the proof, and its necessary and independent character.

44. As the Axioms which are the basis of the Statics of Solids depend upon the idea of body, considered as transmitting force, so the axioms of Hydrostatics depend on the idea of a fluid, considered as a body which transmits pressure in all directions; or, as we may express it more briefly, upon the *idea of fluid pressure*. It is not enough to conceive a fluid as a body, the parts of which are perfectly moveable; for, as I have elsewhere observed\*, "this definition cannot be a sufficient basis for the doctrines of the pressure of fluids; for how can we evolve, out of the mere notion of mobility, which includes no conception of force, the independent conception of pressure." But the conception of fluid as transmitting pressure, supplies us with the requisite axioms. The First Axiom of our Hydrostatics—that if a fluid be contained in a tube of which the two ends are similar and equal planes acted on by equal pressures, it will be kept in equilibrium—follows from the principle of sufficient reason, for there is no reason why either pressure should preponderate. If, for example, the curvature of the tube, or any such cause, affected the pressure at either end, this condition would be a limitation of the property of transmitting pressure in all directions, and would imply imperfect fluidity; whereas the fluidity is supposed to be *perfect*. And for the like reasons, we might assume as an *Axiom* the Third *Proposition* of the Hydrostatics, that fluids transmit pressure *equally* in all directions, from one part of their boundary to the other; for if the pressure transmitted were different according to the direction, this difference might be referred to some cohesion or viscosity of the fluid; and the fluidity might be made more perfect, by conceiving the difference removed. Therefore the proposition would be necessarily and evidently true of a perfect fluid.

45. But instead of laying down this axiom, I have taken the axiom that any part of a fluid which is in equilibrium, may be supposed to become rigid. This axiom leads immediately to the proposition, and it is, besides, of great use in all parts of Hydrostatics. If we had to reason concerning flexible bodies, we might conveniently and properly assume a corresponding axiom for them;—namely, that, of a flexible body which is in

\* *Thoughts on the Study of Mathematics.*

equilibrium, any part may be supposed to become rigid. And we might give a reason for this, by saying that rigidity implies forces which resist a tendency to change of form, when any such tendency occurs; but in a body which is in equilibrium, there is no tendency to change of form, and therefore the resisting forces vanish. It is of no consequence what forces *would* act if there *were* a stress to bend the body: since there *is not* any such stress, the rigidity is not called into play, and therefore it makes no difference whether we suppose it to exist or not.

46. The same kind of reasons may be given, in order to shew the admissibility of introducing, in the case of equilibrium of a fluid, rigidity, instead of that still greater susceptibility of change of figure which fluidity implies. Since the mass is perfectly fluid, its particles exert no constraint on each other's motions; but then, because they are in equilibrium, no constraint is needed to keep them in their places. They are as steadily kept there (so long as the same forces continue to act) as if they were held by the insurmountable forces which connect the parts of a perfectly rigid body. We may therefore suppose the inoperative forces of rigidity to be present or absent among the particles, without altering the other forces or their relations. And hence we see the truth of Axiom 2 of the Hydrostatics.

47. The above considerations (Art. 44) arising from the properties which we assume being perfect, may be applied in other cases; for instance, to shew that the force exerted by a *perfectly* smooth surface is perpendicular to the surface. (*Mech. Euc.* B. 1. Ax. 13.) For if it were not, the force might be resolved into a force perpendicular to the surface, and a force acting along the surface; and the latter force might be referred to some friction or cohesion of the surface. Therefore we should not have supposed the surface perfectly smooth, without imagining this force to vanish: and thus the only force exerted by such a perfectly smooth surface would necessarily be a normal force.

48. The last axiom of Hydrostatics (Ax. 7) is in fact a substitute for an idea which we must exclude in Elementary Mathematics;—the idea of a *Limit*. The attempt to proceed far in Geometry without the use of this idea, gave rise to a series of well-known embarrassments among the ancients. The mode of evading the difficulty which I have adopted, by means

of the axiom just referred to, appeared to me the best. The axiom is readily assented to, if it be considered that, since we may make the particles as small as we please, we may make as small as we please the error arising from the neglect of one particle. We may make it microscopic, and then throw away the microscope; and thus the error vanishes.

49. Some of the Axioms which are stated in Book III., on the Laws of Motion, give occasion to remarks similar to those already made. Thus Axiom 4, which asserts that if particles move in such a manner as always to preserve the same relative distances and positions, their motions will not be altered by supposing them rigidly connected, is evident by the same considerations as the Axioms concerning flexible and fluid bodies, already noticed in Articles 45 and 46. For the forces of rigidity are forces which would prevent a change of the distances and relative positions of the particles if there were a tendency to any such change; and if there be no such tendency, it makes no difference whether the potential resistance to it be present or absent.

50. The 5th Axiom of Book III., which asserts that forces producing parallel and equal velocities at the same time, may be conceived to be added; and the 6th Axiom, which asserts that in systems in motion the action and reaction are equal and opposite, are applications of what is stated in the second sentence of this third Book;—that the Definitions and Axioms of Statics are adopted and assumed in the case of bodies in motion. In the third Book, as in the first, forces are conceived as capable of addition, and matter is conceived as that which can resist force, and transmit it unaltered.

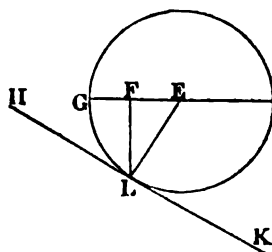
The 3d, 8th, and 9th Axioms of Book III., like the 7th of Book II., are introduced to avoid the reasoning which depends on Limits.

51. In the case of Mechanics, as in the case of Geometry, the distinctness of the idea is necessary to a full apprehension of the truth of the axioms; and in the case of mechanical notions it is far more common than in Geometry, that the axioms are imperfectly comprehended, in consequence of the want of distinctness and exactness in men's ideas. Indeed this indistinctness of mechanical notions has not only prevailed in many individuals at all periods, but we can point out whole centuries, in which it has been, so far as we can trace, universal.

And the consequence of this was, that the science of Statics, after being once established upon clear and sound principles, again fell into confusion, and was not understood as an exact science for two thousand years, from the time of Archimedes to that of Galileo and Stevinus.

52. In order to illustrate this indistinctness of mechanical ideas, I shall take from an ancient Greek writer an attempt to solve a mechanical problem; namely, the Problem of the Inclined Plane. The following is the mode in which Pappus professes\* to answer this question:—"To find the force which will support a given weight  $A$  upon an inclined plane."

Let  $HK$  be the plane; let the weight  $A$  be formed into a sphere: let this sphere be placed in contact with the plane  $HK$ , touching it in the point  $L$ , and let  $E$  be its center. Let  $EG$  be a horizontal radius, and  $LF$  a vertical line which meets it. Take a weight  $B$  which is to  $A$  as  $EF$  to  $FG$ . Then if  $A$  and  $B$  be suspended at  $E$  and  $G$  to the lever  $EFG$  of which the center of motion is  $F$ , they will balance; being supported, as it were, by the fulcrum  $LF$ . And the sphere, which is equal to the weight  $A$ , may be supposed to be collected at its center. If therefore  $B$  act at  $G$ , the weight  $A$  will be supported.



It may be observed that in this attempt, the confusion of ideas is such, that the author assumes a weight which acts at  $G$ , on the lever  $EFG$ , and which is therefore a vertical force, as identical with a force which acts at  $G$ , to support the body in the inclined plane, and which is parallel to the plane.

53. When this kind of confusion was remedied, and when men again acquired distinct notions of pressure, and of the transmission of pressure from one point to another, the science of Statics was formed by Stevinus, Galileo, and their successors†.

The fundamental ideas of Mechanics being thus acquired, and the requisite consequences of them stated in axioms, our

\* Pappus, B. VIII. Prop. ix. I purposely omit the confusion produced by this author's mode of treating the question, in which he inquires the force which will *draw* a body *up* the inclined plane.

† See *History of the Inductive Sciences*, B. VI. chap. i. sect. 2, On the Revival of the Scientific Idea of Pressure.



reasonings proceed by the same rigorous line of demonstration, and under the same logical rules as the reasonings of Geometry; and we have a science of Statics which is, like Geometry, an exact deductive science.

### SECT. II.—*On the Logic of Induction.*

54. THERE are other portions of Mechanics which require to be considered in another manner; for in these there occur principles which are derived directly and professedly from experiment and observation. The derivation of principles by reasoning from facts is performed by a process which is termed *Induction*, which is very different from the process of Deduction already noticed, and of which we shall attempt to point out the character and method.

It has been usual to say of any general truths, established by the consideration and comparison of several facts, that they are obtained by *Induction*; but the distinctive character of this process has not been well pointed out, nor have any rules been laid down which may prescribe the form and ensure the validity of the process, as has been done for Deductive reasoning by common Logic. The *Logic of Induction* has not yet been constructed; a few remarks on this subject are all that can be offered here.

55. The Inductive Propositions, to which we shall here principally refer as examples of their class, are those elementary principles which occur in considering the motion of bodies, and of which some are called the Laws of Motion\*. They are such as these;—a body not acted on by any force will move on for ever uniformly in a straight line;—gravity is a uniform force;—if a body in motion be acted upon by any force, the effect of the force will be compounded with the previous motion;—when a body communicates motion to another directly, the momentum lost by the first body is equal to the momentum gained by the second. And I remark, in the first place, that in collecting such propositions from facts, there occurs a step corresponding to the term “Induction,” (*ἐπαγωγή*, *inductio*). Some notion is *superinduced* upon the observed facts. In each inductive process, there is some general idea introduced, which is given,

\* Inductive Propositions [in the *Mechanical Euclid*] are, Book II. Propositions 25, 26, 32, 36, 37: Book III. Prop. 2, 3, 8, 13.

not by the phenomena, but by the mind. The conclusion is not contained in the premises, but includes them by the introduction of a new generality. In order to obtain our inference, we travel beyond the cases we have before us; we consider them as exemplifications of, or deviations from, some ideal case in which the relations are complete and intelligible. We take a standard, and measure the facts by it; and this standard is created by us, not offered by Nature. Thus we assert, that a body left to itself will move on with unaltered velocity, not because our senses ever disclosed to us a body doing this, but because (taking this as our ideal case) we find that all actual cases are intelligible and explicable by means of the notion of forces which cause change of motion, and which are exerted by surrounding bodies. In like manner, we see bodies striking each other, and thus moving, accelerating, retarding, and stopping each other; but in all this, we do not, by our senses, perceive that abstract quantity, momentum, which is always lost by one as it is gained by another. This momentum is a creation of the mind, brought in among the facts, in order to convert their apparent confusion into order, their seeming chance into certainty, their perplexing variety into simplicity. This the idea of momentum gained and lost does; and, in like manner, in any other case in which inductive truths are established, some idea is introduced, as the means of passing from the facts to the truth.

56. The process of mind of which we here speak can only be described by suggestion and comparison. One of the most common of such comparisons, especially since the time of Bacon, is that which speaks of induction as the *interpretation* of facts. Such an expression is appropriate; and it may easily be seen that it includes the circumstance which we are now noticing;—the superinduction of an idea upon the facts by the interpreting mind. For when we read a page, we have before our eyes only black and white, form and colour; but by an act of the mind, we transform these perceptions into thought and emotion. The letters are nothing of themselves; they contain no truth, if the mind does not contribute its share: for instance, if we do not know the language in which the words are written. And if we are imperfectly acquainted with the language, we become very clearly aware how much a certain activity of the mind is requisite in order to convert the words into propositions, by the extreme

effort which the business of interpretation requires. Induction, then, may be conveniently described as the interpretation of phenomena.

57. But I observe further, that in thus inferring truths from facts, it is not only necessary that the mind should contribute to the task its own idea, but, in order that the propositions thus obtained may have any exact import and scientific value, it is requisite that the idea be perfectly *distinct* and *precise*. If it be possible to obtain some vague apprehension of truths, while the ideas in which they are expressed remain indistinct and ill-defined, such knowledge cannot be available for the purposes we here contemplate. In order to construct a science, all our fundamental ideas must be distinct; and among them, those which Induction introduces.

58. This necessity for distinctness in the ideas which we employ in Induction, makes it proper to *define*, in a precise and exact manner, each idea when it is thus brought forwards. Thus, in establishing the propositions which we have stated as our examples in these cases, we have to define *force* in general; *uniform force*; *compounding* of motions; *momentum*. The construction of these definitions is an essential part of the process of Induction, no less than the assertion of the inductive truth itself.

59. But in order to justify and establish the inference which we make, the ideas which we introduce must not only be distinct, but also *appropriate*. They must be exactly and closely applicable to the facts; so that when the idea is in our possession, and the facts under our notice, we perceive that the former includes and takes up the latter. The idea is only a more precise mode of apprehending the facts, and it is empty and unmeaning if it be anything else; but if it be thus applicable, the proposition which is asserted by means of it is true, precisely because the facts *are* facts. When we have defined force to be the cause of change of motion, we see that, as we remove external forces, we do, in actual experiments, remove all the change of motion; and therefore the proposition that there is in bodies no internal cause of change of motion, is true. When we have defined momentum to be the product of the velocity and quantity of matter, we see that in the actions of bodies, the effect increases as the momentum increases; and by measurement, we find that the effect may consistently be measured by the momen-

tum. The ideas here employed are not only distinct in the mind, but applicable in the world; they are the elements, not only of relations of thought, but of laws of nature.

60. Thus an inductive inference requires an idea from within, facts from without, and a coincidence of the two. The idea must be distinct, otherwise we obtain no scientific truth; it must be appropriate, otherwise the facts cannot be steadily contemplated by means of it; and when they are so contemplated, the Inductive Proposition must be seen to be verified by the evidence of sense.

It appears from what has been said, that in establishing a proposition by Induction, the definition of the idea and the assertion of the truth, are not only both requisite, but they are correlative. Each of the two steps contains the verification and justification of the other. The proposition derives its meaning from the definition; the definition derives its reality from the proposition. If they are separated, the definition is arbitrary or empty, the proposition is vague or verbal.

61. Hence we gather, that in the Inductive Sciences, our Definitions and our Elementary Inductive Truths ought to be introduced together. There is no value or meaning in definitions, except with reference to the truths which they are to express. Discussions about the definitions of any science, taken separately, cannot therefore be profitable, if the discussion do not refer, tacitly or expressly, to the fundamental truths of the science; and in all such discussions it should be stated what are taken as the fundamental truths. With such a reference to Elementary Inductive Truths clearly understood, the discussion of Definitions may be the best method of arriving at that clearness of thought, and that arrangement of facts, which Induction requires.

I will now note some of the differences which exist between Inductive and Deductive Reasoning, in the modes in which they are presented.

62. One leading difference in these two kinds of reasoning is, that in Deduction we infer particular from general truths; in Induction, on the contrary, we infer general from particular. Deductive proofs consist of many steps, in each of which we apply known general propositions in particular cases;—"all triangles have their angles equal to two right angles, therefore this triangle has; therefore, &c." In Induction, on the other hand, we have a single step in which we pass from many par-

ticular Propositions to one general proposition; "This stone falls downwards; so do those others;—all stones fall downwards." And the former inference flows necessarily from the relation of general and particular; but the latter, as we have seen, derives its power of convincing from the introduction of a new idea, which is distinct and appropriate, and which supplies that generality which the particulars cannot themselves offer.

63. I observe also that this difference of process in inductive and deductive proofs, may be most properly marked by a difference in the form in which they are stated. In Deduction, the *Definition* stands at the beginning of the proposition; in Induction, it may most suitably stand at or near the end. Thus the definition of a uniform force is introduced in the course of the proposition that gravity is a uniform force. And this arrangement represents truly the real order of proof; for, historically speaking, it was taken for granted that gravity was a uniform force; but the question remained, what was the right definition of a uniform force. And in the establishment of other inductive principles, in like manner, definitions cannot be laid down for any useful purpose, till we know the propositions in which they are to be used. They may therefore properly come each at the conclusion of its corresponding proposition.

64. The ideas and definitions which are thus led to by our inductive process, may bring with them Axioms. Such Axioms may be self-evident as soon as the inductive idea has been distinctly apprehended, in the same manner as was explained respecting the fundamental ideas of Geometry and Statics. And thus *Axioms*, as well as *Definitions*, may come at the end of our Inductive Propositions; and they thus assume their proper place at the beginning of the deductive propositions which follow them, and are proved from them. Thus, in Book III., *Axioms* 8 and 9, come after the definition of Accelerating Force, and stand between Props. 14 and 15.

65. Another peculiarity in inductive reasoning may be noticed. In a deductive demonstration, the reference is always to what has been already proved; in establishing an Inductive Principle, it is most convenient that the reference should be to subsequent propositions. For the proof of the Inductive Principle consists in this;—that the principle being adopted, consequences follow which agree with fact; but the demonstration of these consequences may require many steps, and several special

propositions. Thus the Inductive Principle, that gravity is a uniform force, is established by shewing that the law of descent, which falling bodies follow in fact, is explained by means of this principle; namely, the law that the space is as the square of the time from the beginning of the motion. But the proof of such a property, from the definition of a uniform force, requires many steps, as may be seen [in the *Mechanical Euclid*], B. III. Pr. 5: and this proof must be referred to, along with several others, in order to establish the truth, that gravity is a uniform force.

66. It may be suggested, that, this being the case, the propositions might be transposed, so that the inductive proof might come after those propositions to which it refers. But if this were done, all the propositions which depend upon the laws of motion must be proved hypothetically only. For instance, we must say, "If, in the communication of motion, the momentum lost and gained be equal, the velocity acquired by a body falling down an inclined plane, will be equal to that acquired by falling down the height." This would be inconvenient, and even if it were done, that completeness in the line of demonstration which is the object of the change, could not be obtained; for the transition from the particular cases to the general truth, which must occur in the Inductive Proposition, could not be in any way justified according to rules of Deductive Logic.

I have, therefore, in the preceding pages, placed the Inductive Principle first in each line of reasoning; and have ranged after it the Deductions from it, which justify and establish it as their first office, but which are more important as its consequences and applications, after it is supposed to be established.

67. I have used one common *formula* in presenting the proof of each of the Inductive Principles which I have introduced;—namely, after stating or exemplifying the facts which the induction includes, I have added "These results can be clearly explained and rigorously deduced by introducing the *Idea* or the *Definition*," which belongs to each case, "and the *Principle*," which expresses the inductive truth. I do not mean to assert that this formula is the only right one, or even the best; but it appears to me to bring under notice the main circumstances which render an induction systematic and valid.

68. It may be observed, however, that this formula does not express the full cogency of the proof. It declares only that the results *can* be clearly explained and rigorously deduced by

the employment of a certain definition and a certain proposition. But in order to make the conclusion demonstrative, we ought to be able to declare that the results can be clearly explained and rigorously deduced *only* by the definition and proposition which we adopt. And, in reality, the mathematician's conviction of the truth of the Laws of Motion does depend upon his seeing that they (or laws equivalent to them) afford the *only* means of clearly expressing and deducing the actual facts. But this conviction, that no other law than those proposed can account for the known facts, finds its place in the mind gradually, as the contemplation of the consequences of the law and the various relations of the facts becomes steady and familiar. I have therefore not thought it proper to require such a conviction along with the first assent to the inductive truths which I have here stated.

69. The propositions established by Induction are termed *Principles*, because they are the starting points of trains of deductive reasoning. In the system of deduction, they occupy the same place as axioms; and accordingly they are termed so by Newton—"Axiomata sive leges motus." Stewart objects strongly to this expression\*: and it would be difficult to justify it; although to draw the line between axioms and inductive principles may be a harder task than at first appears.

70. But from the consideration that our Inductive Propositions are the principles or beginnings of our deductive reasoning, and so far at least stand in the place of axioms, we may gather *this* lesson,—that they are not to be multiplied without necessity. For instance, if in a treatise on Hydrostatics, we should state as two separate propositions, that "air has weight;" and that "the mercury in the barometer is sustained by the weight of the air;" and should prove both the one and the other by reference to experiment; we should offend against the maxims of Logic. These propositions are connected; the latter may be demonstrated deductively from the former; the former may be inferred inductively from the facts which prove the latter. One of these two courses ought to be adopted; we ought not to have two ends of our reasoning upwards, or two beginnings of our reasoning downwards.

71. I shall not now extend these Remarks further. They may appear to many barren and unprofitable speculations; but

\* *Elem. Phil. Human Mind*, Vol. II. p. 44.

those who are familiar with such subjects, will perhaps find in them something which, if well founded, is not without some novelty for the English reader. Such will, I think, be the case, if I have satisfied him,—that mathematical truth depends on axioms as well as definitions,—that the evidence of geometrical axioms is to be found only in the distinct possession of the idea of space,—that other branches of mathematics also depend on axioms,—and that the evidence of these axioms is to be sought in some appropriate idea ;—that the evidence of the axioms of statics, for instance, resides in the ideas of force and matter ;—that in the process of induction the mind must supply an idea in addition to the facts apprehended by the senses ;—that in each such process we must introduce one or more definitions, as well as a proposition ;—that the definition and the proposition are correlative, neither being useful or valid without the other ;—and that the formula of inductive reasoning must be in many respects the reverse of the common logical formulæ of deduction.

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## ESSAY III.

### DEMONSTRATION THAT ALL MATTER IS HEAVY\*.

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THE discussion of the nature of the grounds and proofs of the most general propositions which the physical sciences include, belongs rather to Metaphysics than to that course of experimental and mathematical investigation by which the sciences are formed. But such discussions seem by no means unfitted to occupy the attention of the cultivators of physical science. The ideal, as well as the experimental side of our knowledge must be carefully studied and scrutinized, in order that its true import may be seen; and this province of human speculation has been perhaps of late unjustly depreciated and neglected by men of science. Yet it can be prosecuted in the most advantageous manner by them only: for no one can speculate securely and rightly respecting the nature and proofs of the truths of science without a steady possession of some large and solid portions of such truths. A man must be a mathematician, a mechanical philosopher, a natural historian, in order that he may philosophize well concerning mathematics, and mechanics, and natural history; and the mere metaphysician who without such preparation and fitness sets himself to determine the grounds of mathematical or mechanical truths, or the principles of classification, will be liable to be led into error at every step. He must speculate by means of general terms, which he will not be able to use as instruments of discovering and conveying philosophical truth, because he cannot, in his own mind, habitually and familiarly, embody their import in special examples.

Acting upon such views, I have already laid before the Philosophical Society of Cambridge essays on such subjects as I here refer to; especially a memoir "On the Nature of the Truth of the Laws of Motion," which was printed by the Society in its Transactions. This memoir appears to have

\* From the *Transactions of the Cambridge Philosophical Society*, Vol. VII. Part II. No. 12. [1841.]

excited in other places, notice of such a kind as to shew that the minds of many speculative persons are ready for and inclined towards the discussion of such questions. I am therefore the more willing to bring under consideration another subject of a kind closely related to the one just mentioned.

The general questions which all such discussions suggest, are (in the existing phase of English philosophy) whether certain proposed scientific truths, (as the laws of motion,) be *necessary* truths; and if they are necessary, (which I have attempted to shew that in a certain sense they are,) *on what ground* their necessity rests. These questions may be discussed in a general form, as I have elsewhere attempted to shew. But it may be instructive also to follow the general arguments into the form which they assume in special cases; and to exhibit, in a distinct shape, the incongruities into which the opposite false doctrine leads us, when applied to particular examples. This accordingly is what I propose to do in the present memoir, with regard to the proposition stated at the head of this Essay, namely, that *all matter is heavy*.

At first sight it may appear a doctrine altogether untenable to assert that this proposition is a necessary truth: for, it may be urged, we have no difficulty in conceiving matter which is not heavy; so that matter without weight is a conception not inconsistent with itself; which it must be if the reverse were a necessary truth. It may be added, that the possibility of conceiving matter without weight was shewn in the controversy which ended in the downfall of the phlogiston theory of chemical composition; for some of the reasoners on this subject asserted phlogiston to be a body with positive levity instead of gravity, which hypothesis, however false, shews that such a supposition is possible. Again, it may be said that *weight* and *inertia* are two separate properties of matter: that mathematicians measure the quantity of matter by the inertia, and that we learn by experiment only that the weight is proportional to the inertia; Newton's experiments with pendulums of different materials having been made with this very object.

I proceed to reply to these arguments. And first, as to the possibility of conceiving matter without weight, and the argument thence deduced, that the universal gravity of matter is not a necessary truth, I remark, that it is indeed just, to say that we cannot even distinctly conceive the contrary of a

necessary truth to be true; but that this impossibility can be asserted only of those perfectly distinct conceptions which result from a complete development of the fundamental idea and its consequences. Till we reach this stage of development, the obscurity and indistinctness may prevent our perceiving absolute contradictions, though they exist. We have abundant store of examples of this, even in geometry and arithmetic; where the truths are universally allowed to be necessary, and where the relations which are impossible, are also inconceivable, that is, not conceivable distinctly. Such relations, though not distinctly conceivable, still often appear conceivable and possible, owing to the indistinctness of our ideas. Who, at the first outset of his geometrical studies, sees any impossibility in supposing the side and the diagonal of a square to have a common measure? Yet they can be rigorously proved to be incommensurable, and therefore the attempt distinctly to conceive a common measure of them must fail. The attempts at the geometrical duplication of the cube, and the supposed solutions, (as that of Hobbes) have involved absolute contradictions; yet this has not prevented their being long and obstinately entertained by men, even of minds acute and clear in other respects. And the same might be shewn to be the case in arithmetic. It is plain, therefore, that we cannot, from the supposed possibility of conceiving matter without weight, infer that the contrary may not be a necessary truth.

Our power of judging, from the compatibility or incompatibility of our conceptions, whether certain propositions respecting the relations of ideas are true or not, must depend entirely, as I have said, upon the degree of development which such ideas have undergone in our minds. Some of the relations of our conceptions on any subject are evident upon the first steady contemplation of the fundamental idea by a sound mind: these are the *axioms* of the subject. Other propositions may be deduced from the axioms by strict logical reasoning. These propositions are no less *necessary* than the axioms, though to common minds their *evidence* is very different. Yet as we become familiar with the steps by which these ulterior truths are deduced from the axioms, *their* truth also becomes evident, and the contrary becomes inconceivable. When a person has familiarized himself with the first twenty-six propositions of Euclid, and not till then, it becomes evident to him, that paral-

lelograms on the same base and between the same parallels are equal; and he cannot even conceive the contrary. When he has a little further cultivated his geometrical powers, the equality of the square on the hypotenuse of a right-angled triangle to the squares on the sides, becomes also evident; the steps by which it is demonstrated being so familiar to the mind as to be apprehended without a conscious act. And thus, the contrary of a necessary truth cannot be distinctly conceived; but the incapacity of forming such a conception is a condition which depends upon cultivation, being intimately connected with the power of rapidly and clearly perceiving the connection of the necessary truth under consideration with the elementary principles on which it depends. And thus, again, it may be that there is an absolute impossibility of conceiving matter without weight; but then, this impossibility may not be apparent, till we have traced our fundamental conceptions of matter into some of their consequences.

The question then occurs, whether we can, by any steps of reasoning, point out an inconsistency in the conception of matter without weight. This I conceive we may do, and this I shall attempt to shew.

The general mode of stating the argument is this:—the quantity of matter is measured by those sensible properties of matter which undergo quantitative addition, subtraction and division, as the matter is added, subtracted and divided. The quantity of matter cannot be known in any other way. But this mode of measuring the quantity of matter, in order to be true at all, must be universally true. If it were only partially true, the limits within which it is to be applied would be arbitrary; and therefore the whole procedure would be arbitrary, and, as a method of obtaining philosophical truth, altogether futile.

We may unfold this argument further. Let the contrary be supposed, of that which we assert to be true: namely, let it be supposed that while all other kinds of matter are heavy, (and of course heavy in proportion to the quantity of matter) there is one kind of matter which is absolutely destitute of weight; as, for instance, phlogiston, or any other element. Then where this *weightless* element (as we may term it) is mixed with *weighty* elements, we shall have a compound, in which the weight is no longer proportional to the quantity of matter. If, for example, 2 measures of heavy matter unite with 1 measure of phlogiston,

the weight is as 2, and the quantity of matter as 3. In all such cases, therefore, the weight ceases to be the measure of the quantity of matter. And as the proportion of the weighty and the weightless matter may vary in innumerable degrees in such compounds, the weight affords no criterion at all of the quantity of matter in them. And the smallest admixture of the weightless element is sufficient to prevent the weight from being taken as the measure of the quantity of matter.

But on this hypothesis, how are we to distinguish such compounds from bodies consisting purely of heavy matter? How are we to satisfy ourselves that there is not, in every body, some admixture, small or great, of the weightless element? If we call this element *phlogiston*, how shall we know that the bodies with which we have to do are, any of them, absolutely free from phlogiston?

We cannot refer to the weight for any such assurance; for by supposition the presence and absence of phlogiston makes no difference in the weight. Nor can any other properties secure us at least from a very small admixture; for to assert that a mixture of 1 in 100 or 1 in 10 of phlogiston would always manifest itself in the properties of the body, must be an arbitrary procedure, till we have proved this assertion by experiment: and we cannot do this till we have learnt some mode of measuring the quantities of matter in bodies and parts of bodies; which is exactly what we question the possibility of, in the present hypothesis.

Thus, if we assume the existence of an element, *phlogiston*, devoid of weight, we cannot be sure that every body does not contain some portion of this element; while we see that if there be an admixture of such an element, the weight is no longer any criterion of the quantity of matter. And thus we have proved, that if there be any kind of matter which is not heavy, the weight can no longer avail us, *in any case or to any extent*, as a measure of the quantity of matter.

I may remark, that the same conclusion is easily extended to the case in which phlogiston is supposed to have absolute levity; for in that case, a certain mixture of phlogiston and of heavy matter would have no weight, and might be substituted for phlogiston in the preceding reasoning.

I may remark, also, that the same conclusion would follow by the same reasoning, if any kind of matter, instead of being

void of weight, were heavy, indeed, but not *so* heavy, in proportion to its quantity of matter, as other kinds.

On all these hypotheses there would be no possibility of measuring quantity of matter by weight at all, in any case, or to any extent.

But it may be urged, that we have not yet reduced the hypothesis of matter without weight to a contradiction; for that mathematicians measure quantity of matter, not by weight, but by the other property, of which we have spoken, inertia.

To this I reply, that, practically speaking, quantity of matter is always measured by weight, both by mechanicians and chemists: and as we have proved that this procedure is utterly insecure in all cases, on the hypothesis of weightless matter, the practice rests upon a conviction that the hypothesis is false. And yet the practice is universal. Every experimenter measures quantity of matter by the balance. No one has ever thought of measuring quantity of matter by its inertia practically: no one has constructed a measure of quantity of matter in which the matter produces its indications of quantity by its motion. When we have to take into account the inertia of a body, we inquire what its weight is, and assume this as the measure of the inertia; but we never take the contrary course, and ascertain the inertia first in order to determine by that means the weight.

But it may be asked, Is it not then true, and an important scientific truth, that the *quantity of matter* is measured by the *inertia*? Is it not true, and proved by experiment, that the *weight* is *proportional* to the *inertia*? If this be not the result of Newton's experiments mentioned above, what, it may be demanded, do they prove?

To these questions I reply: It is true that quantity of matter is measured by the inertia, for it is true that inertia is as the quantity of matter. This truth is indeed one of the laws of motion. That weight is proportional to inertia is proved by experiment, as far as the laws of motion are so proved: and Newton's experiments prove one of the laws of motion, so far as any experiments can prove them, or are needed to prove them.

That inertia is proportional to weight, is a law equivalent to that law which asserts, that when pressure produces motion in a given body, the velocity produced in a given time is as the pressure. For if the velocity be as the pressure, when the body

is given, the velocity will be constant if the inertia also be as the pressure. For the inertia is understood to be that property of bodies to which, *ceteris paribus*, the velocity impressed is *inversely* proportional. One body has twice as much inertia as another, if, when the same force acts upon it for the same time, it acquires but half the velocity. This is the fundamental conception of *inertia*.

In Newton's pendulum experiments, the pressure producing motion was a certain resolved part of the weight, and was proportional to the weight. It appeared by the experiments, that whatever were the material of which the pendulum was formed, the rate of oscillation was the same; that is, the velocity acquired was the same. Hence the inertia of the different bodies must have been in each case as the weight: and thus this assertion is true of all different kinds of bodies.

Thus it appears that the assertion, that inertia is universally proportional to weight, is equivalent to the law of motion, that the velocity is as the pressure. The conception of inertia (of which, as we have said, the fundamental conception is, that the velocity impressed is inversely proportional to the inertia,) connects the two propositions so as to make them identical.

Hence our argument with regard to the universal gravity of matter brings us to the above law of motion, and is proved by Newton's experiments in the same sense in which that law of motion is so proved.

Perhaps some persons might conceive that the identity of weight and inertia is obvious at once; for both are merely resistance to motion;—inertia, resistance to all motion (or change of motion)—weight, resistance to motion upwards.

But there is a difference in these two kinds of resistance to motion. Inertia is instantaneous, weight is continuous resistance. Any momentary impulse which acts upon a free body overcomes its inertia, for it changes its motion; and this change once effected, the inertia opposes any return to the former condition, as well as any additional change. The inertia is thus overcome by a momentary force. But the weight can only be overcome by a continuous force like itself. If an impulse act in opposition to the weight, it may for a moment neutralize or overcome the weight; but if it be not continued, the weight resumes its effect, and restores the condition which existed before the impulse acted.

But weight not only produces rest, when it is resisted, but motion, when it is not resisted. Weight is measured by the reaction which would balance it; but when unbalanced, it produces motion, and the velocity of this motion increases constantly. Now what determines the velocity thus produced in a given time, or its rate of increase? What determines it to have one magnitude rather than another? To this we must evidently reply, *the inertia*. When weight produces motion, the inertia is the reaction which makes the motion determinate. The accumulated motion produced by the action of unbalanced weight is as determinate a condition as the equilibrium produced by balanced weight. In both cases the condition of the body acted on is determined by the opposition of the action and reaction.

Hence inertia is the reaction which opposes the weight, when unbalanced. But by the conception of action and reaction, (as mutually determining and determined,) they are measured by each other: and hence the inertia is necessarily proportional to the weight.

But when we have reached this conclusion, the original objection may be again urged against it. It may be said, that there must be some fallacy in this reasoning, for it proves a state of things to be necessary when we can so easily conceive a contrary state of things. Is it denied, the opponent may ask, that we can readily imagine a state of things in which bodies have no weight? Is not the uniform tendency of all bodies in the same direction not only not necessary, but not even true? For they do in reality tend, not with equal forces in parallel lines, but to a center with unequal forces, according to their position: and we can conceive these differences of intensity and direction in the force to be greater than they really are; and can with equal ease suppose the force to disappear altogether.

To this I reply, that certainly we may conceive the weight of bodies to vary in intensity and direction, and by an additional effort of imagination, may conceive the weight to vanish: but that in all these suppositions, even in the extreme one, we must suppose the rule to be universal. If *any* bodies have weight, *all* bodies must have weight. If the direction of weight be different in different points, this direction must still vary according to the *law of continuity*; and the same is true of the intensity of the weight. For if this were not so, the rest and motion, the velocity and direction, the permanence and change of bodies,



as to their mechanical condition, would be arbitrary and incoherent: they would not be subject to mechanical ideas; that is, not to ideas at all: and hence these conditions of objects would in fact be inconceivable. In order that the universe may be possible, that is, may fall under the conditions of intelligible conceptions, we must be able to conceive a body at rest. But the rest of bodies (except in the absolute negation of all force) implies the equilibrium of opposite forces. And one of these opposite forces must be a *general* force, as weight, in order that the universe may be governed by general conditions. And this general force, by the conception of force, may produce motion, as well as equilibrium; and this motion again must be determined, and determined by general conditions; which cannot be, except the communication of motion be regulated by an inertia proportional to the weight.

But it will be asked, Is it then pretended that Newton's experiment, by which it was intended to prove inertia proportional to weight, does really prove nothing but what may be demonstrated *à priori*? Could we know, without experiment, that all bodies,—gold, iron, wood, cork,—have inertia proportional to their weight? And to this we reply, that experiment holds the same place in the establishment of this, as of the other fundamental doctrines of mechanics. Intercourse with the external world is requisite for developing our ideas; measurement of phenomena is needed to fix our conceptions and to render them precise: but the result of our experimental studies is, that we reach a position in which our convictions do not rest upon experiment. We learn by observation truths of which we afterwards see the necessity. This is the case with the laws of motion, as I have repeatedly endeavoured to shew. The same will appear to be the case with the proposition, that bodies of different kinds have their inertia proportional to their weight.

For bodies of *the same kind* have their inertia proportional to their weight, both quantities being proportional to the quantity of matter. And if we compress the same quantity of matter into half the space, neither the weight nor the inertia is altered, because these depend on the quantity of matter alone. But in this way we obtain a body of *twice the density*; and in the same manner we obtain a body of any other density. Therefore whatever be the density, the inertia is proportional to the quantity of matter. But the mechanical relations of bodies cannot

depend upon any difference of *kind*, *except* a difference of density. For if we suppose any fundamental difference of mechanical nature in the particles or component elements of bodies, we are led to the same conclusion, of arbitrary, and therefore impossible, results, which we deduced from this supposition with regard to weight. Therefore all bodies of different density, and hence, all bodies whatever, must have their inertia proportional to their weight.

Hence we see, that the propositions, that all bodies are heavy, and that inertia is proportional to weight, necessarily follow from those fundamental ideas which we unavoidably employ in all attempts to reason concerning the mechanical relations of bodies. This conclusion may perhaps appear the more startling to many, because they have been accustomed to expect that fundamental ideas and their relations should be self-evident at our first contemplation of them. This, however, is far from being the case, as I have already shewn. It is not the *first*, but the most complete and developed condition of our conceptions, which enables us to see what are axiomatic truths in each province of human speculation. Our fundamental ideas are necessary conditions of knowledge, universal forms of intuition, inherent types of mental development; they may even be termed, if any one chooses, results of connate intellectual tendencies; but we cannot term them *innate* ideas, without calling up a large array of false opinions. For innate ideas were considered as capable of composition, but by no means of simplification: as most perfect in their original condition; as to be found, if any where, in the most uneducated and most uncultivated minds; as the same in all ages, nations, and stages of intellectual culture; as capable of being referred to at once, and made the basis of our reasonings, without any special acuteness or effort: in all which circumstances the Fundamental Ideas of which we have spoken, are opposed to Innate Ideas so understood.

I shall not, however, here prosecute this subject. I will only remark, that Fundamental Ideas, as we view them, are not only not innate, in any usual or useful sense, but they are not necessarily *ultimate* elements of our knowledge. They are the results of our analysis so far as we have yet prosecuted it; but they may themselves subsequently be analysed. It may hereafter appear, that what we have treated as different Fundamental Ideas have, in fact, a connexion, at some point below the

structure which we erect upon them. For instance, we treat of the mechanical ideas of force, matter, and the like, as distinct from the idea of substance. Yet the principle of measuring the quantity of matter by its weight, which we have deduced from mechanical ideas, is applied to determine the substances which enter into the composition of bodies. The idea of substance supplies the axiom, that the whole quantity of matter of a compound body is equal to the sum of the quantities of matter of its elements. The mechanical ideas of force and matter lead us to infer that the quantity both of the whole and its parts must be measured by their weights. *Substance* may, for some purposes, be described as that to which properties belong; *matter* in like manner may be described as that which resists force. The former involves the Idea of permanent Being; the latter, the Idea of Causation. There may be some elevated point of view from which these ideas may be seen to run together. But even if this be so, it will by no means affect the validity of reasonings founded upon these notions, when duly determined and developed. If we once adopt a view of the nature of knowledge which makes necessary truth possible at all, we need be little embarrassed by finding how closely connected different necessary truths are; and how often, in exploring towards their roots, different branches appear to spring from the same stem.

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## ESSAY IV.

### DISCUSSION OF THE QUESTION:—ARE CAUSE AND EFFECT SUCCESSIVE OR SIMULTANEOUS?\*

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I HAVE at various times laid before this Society dissertations on the metaphysical grounds and elements of our knowledge, and especially on the foundations of the science of Mechanics. As these speculations have not failed to excite some attention, both here and elsewhere, I am tempted to bring forward in the same manner some additional disquisitions of the same kind. Indeed, the immediate occasion of the present memoir is of itself an evidence that such subjects are not supposed to be without their interest for the general reader; for I am led to the views and reasonings which I am now about to lay before the Society, by some remarks in one of our most popular Reviews, (*The Quarterly Review*, Article on the History and Philosophy of the Inductive Sciences. June 1841.) A writer of singular acuteness and comprehensiveness of view has there made remarks upon the doctrines which I had delivered in the "Philosophy of the Inductive Sciences," which remarks appear to me in the highest degree instructive and philosophical. I am not, however, going here to discuss fully the doctrines contained in this critique. With respect to its general tenency, I will only observe, that the author does not accept, in the form in which I had given it, the account of the origin and ground of necessary and universal truths. I had stated that our knowledge is derived from Sensations and Ideas; and that Ideas, which are the conditions of perception, such as *space*, *time*, *likeness*, *cause*, make universal and necessary knowledge possible; whereas, if knowledge were derived from Sensation alone, it could not have those characters. I have moreover enumerated a long series of Fundamental Ideas as the bases of a corresponding series of sciences, of which sciences I have shown also, by an historical survey, that they claim to possess universal truths, and have their claims allowed. I have gone

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further: for I have stated the Axioms which flow from these Fundamental Ideas, and which are the logical grounds of necessity and universality in the truths of each science, when the science is presented in the form of a demonstrated system. The Reviewer does not assent to this doctrine, nor to the argument by which it is supported; namely, that Experience cannot lead to universal truths, except by means of a universal Idea supplied by the mind, and infused into the particular facts which observation ministers. He considers that the existence of universal truths in our knowledge may be explained otherwise. He holds that it is a sufficient account of the matter to say that we pass from special experience to universal truth in virtue of "the inductive propensity—the irresistible impulse of the mind to generalize *ad infinitum*." I shall not here dwell upon very strong reasons which may be assigned, as I conceive, for not accepting this as a full and satisfactory explanation of the difficulty. Instead of doing so, I shall here content myself with remarking, that even if we adopt the Reviewer's expressions, we must still contend that there are *different forms* of the *impulse of the mind to generalize*, corresponding to each of the Fundamental Ideas of our system. These Fundamental Ideas, if they be nothing else, must at least be accepted as a classification of the modes of action of the Inductive Propensity,—as so many different paths and tendencies of the Generalizing Impulse: and the Axioms which I have stated as the express results of the Fundamental Ideas, and as the steps by which those Ideas make universal truths possible, are still no less worthy of notice, if they are stated as the results of our Generalizing Impulse; and as the steps by which that Impulse, in its many various forms, makes universal truths possible. The Generalizing Impulse in that operation by which it leads us to the Axioms of geometry, and to those of mechanics, takes very different courses; and these courses may well deserve to be separately studied. And perhaps, even if we accept this view of the philosophy of our knowledge, no simpler or clearer way can be found of describing and distinguishing these fundamentally different operations of the Inductive Propensity, than by saying, that in the one case it proceeds according to the Idea of Space, in another according to the Idea of Mechanical Cause; and the like phraseology may be employed for all the other cases.

This then being understood, my present object is to consider

some very remarkable, and, as appears to me, novel views of the Idea of Cause which the Reviewer propounds. And these may be best brought under our discussion by considering them as an attempt to solve the question, Whether, according to our fundamental apprehensions of the relation of Cause and Effect, effect follows cause in the order of time, or is simultaneous with it.

At first sight, this question may seem to be completely decided by our fundamental convictions respecting cause and effect, and by the axioms which have been propounded by almost all writers, and have obtained universal currency among reasoners on this subject. That the cause must precede the effect,—that the effect must follow the cause,—are, it might seem, self-evident truths, assumed and assented to by all persons in all reasonings in which those notions occur. Such a doctrine is commonly asserted in general terms, and seems to be verified in all the applications of the idea of cause. A heavy body produces motion by its weight; the motion produced is subsequent in time to the pressure which the weight exerts. In a machine, bodies push or strike each other, and so produce a series of motions; each motion, in this case, is the result of the motions and configurations which have preceded it. The whole series of such motions employs time; and this time is filled up and measured by the series of causes and effects, the effects being, in their turn, causes of other effects. This is the common mode of apprehending the universal course of events, in which the chain of causation, and the progress of time, are contemplated as each the necessary condition and accompaniment of the other.

But this, the Critic remarks, is not true in *direct* causation. “If the antecedence and consequence in question be understood as the interposition of an interval of time, however small, between the action of the cause and the production of the effect, we regard it as inadmissible. In the production of motion by force, for instance, though the effect be cumulative with continued exertion of the cause, yet each elementary or individual action is, to our apprehension, *instantaneous* accompanied with its corresponding increment of momentum in the body moved. In all dynamical reasonings no one has ever thought of interposing an instant of time between the action and its resulting momentum; nor does it appear necessary.” This is so evident, that it

appears strange it should have the air of novelty ; yet, so far as I am aware, the matter has never before been put in the same point of view. But this being the case, the question occurs, how it is that time *seems* to be employed in the progress from cause to effect ! How is it that the opinion of the effect being subsequent to the cause has generally obtained ! And to this the Critic's answer is obvious :—it is so in cases of indirect or of *cumulative* effect. If a ball *A* strikes another, *B*, and puts it in motion, and *B* strikes *C*, and puts it in motion, *A*'s impact may be considered as the cause, though not the direct cause, of *C*'s motion. Now time, namely the time of *B*'s motion after it is struck by *A*, and before it strikes *C*, intervenes between *A*'s impact and the beginning of *C*'s motion : that is, between the cause and its effect. In this sense, the effect is subsequent to the cause. Again, if a body be put in motion by a series of impulses acting at finite intervals of time, all in the same direction, the motion at the end of all these intervals is the effect of all the impulses, and exists after they have all acted. It is the accumulated effect, and subsequent to each separate action of the cause. But in this case, each impulse produces its effect instantaneously, and the time is employed, not in the transition from any cause to its effect, but in the intervals between the action of the several causes, during which intervals the body goes on with the velocity already communicated to it. In each impulse, force produces motion : and the motion goes on till a new change takes place, by the same kind of action. The force may be said, in the language employed by the Critic, to be transformed into momentum ; and in the successive impulses, successive portions of force are thus transformed ; while in the intervening intervals, the force thus transformed into momentum is carried by the body from one place to another, where a new change awaits it. “The cause is absorbed and transformed into effect, and therein treasured up.” Hence, as the Writer says, “The time lost in cases of indirect physical causation is that consumed in the movements which take place among the parts of the mechanism set in action, by which the active forces so transformed into mechanism are transported over intervals of space to new points of action, the motion of matter in such cases being regarded as a mere carrier of force” :—and when force is directly counteracted by force, their mutual destruction must be conceived, as the Reviewer says, to be instantaneous.

We can therefore hardly resist his conclusion, that men have been misled in assuming sequence as a feature in the relation of cause and effect; and we may readily assent to his suggestion, that sequence, when observed, is to be held as a sure indication of indirect action, accompanied with a movement of parts.

But yet if we turn for a moment to other kinds of causation, we seem to be compelled at every step to recognize the truth of the usual maxim upon this subject, that effects are subsequent to causes. Is not poison, taken at a certain moment, the cause of disorder and death which follow at a *subsequent* period? Is not a man's early prudence often the cause of his prosperity in *later* life, and his folly, though for a moment it may produce gratification, *finally* the cause of his ruin? And even in the case of mechanism, if, in a clock which goes rightly, we alter the length of the pendulum, is not this alteration the cause of an alteration which *afterwards* takes place in the rate of the clock's going? Are not all these, and innumerable other cases, instances in which the usual notion of the effect following the cause is verified? and are they not irreconcilable with the new doctrine of cause and effect being simultaneous?

In order to disentangle this apparent confusion, let us first consider the case last mentioned, of a clock, in which some alteration is made which affects the rate of going.

So long as the parts of the clock remain unaltered, its rate will remain unaltered; and any part which is considered as capable of alteration, may be considered as, if we please, the cause of the unaltered rate, by being itself unaltered. But we do not usually introduce the positive idea of cause, to correspond with this negation of change. If we speak of the rate as unaltered, we may also say that it is so because there is *no cause* of alteration. The steady rate is the indication of the absence of any cause of alteration; and the rate of going measures the progress of time, in a state of things in which causes of change are thus excluded. If an alteration takes place in any part of the clock, once for all, the rate is altered; but the new rate is steady as the old rate was, and, like it, measures the uniform progress of time. But the difference between the new rate and the old is occasioned by the difference of the parts of the clock; and the new rate may very properly be said to be caused by the change of the parts, and to be subsequent to it: for it does prevail after the change, and does not prevail before.



But how is this view to be reconciled with the one just quoted from the Reviewer, and, as it appeared, satisfactorily proved by him; according to which all mechanical effects are simultaneous with their causes, and not subsequent to them! We have here the two views in close contact, and in seeming opposition.

In the going of a clock, the parts are in motion; and these motions are determined by forces arising from the form and connexion of the parts of the mechanism. Each of the forces thus exerted at any instant produces its effect at the same instant; and thus, so far as the term *cause* refers to such instantaneous forces, the cause and the effect are simultaneous. But if such instantaneous forces act at successive intervals of time, the motion during each interval is unaltered, and by its uniform progress measures the progress of time. And thus the motion of the machine consists of a series of intervals, during each of which the motion is uniform, and measures the time; separated from each other by a series of changes, at each of which the change measures the instantaneous force, and is simultaneous with it. And if, in this case, we suppose, at any point of time, the instantaneous forces to cease, the succession of them being terminated, from that point of time the motion would be uniform. And since the rate of the motion in each interval of time is determined by the instantaneous force which last acted and by the preceding motion, the rate of the motion in each interval of time is determined by all the preceding instantaneous forces. Hence, when the series of instantaneous forces stops, the rate at which the motion goes on permanently, from that point of time, is determined by the antecedent series of such forces, which series may be considered as an aggregate cause; and hence it appears, that the *permanent* effect is determined by the *aggregate* cause; and in this sense the effect is subsequent to the cause.

Thus we obtain, in this case, a solution of the difficulty which is placed before us. The instantaneous effect or change is simultaneous with the instantaneous force or cause by which it is produced. But if we consider a series of such instantaneous forces as a single aggregate cause, and the final condition as a permanent effect of this cause, the effect is subsequent to the cause. In this case, the cause is immediately succeeded by the effect. The cause acts in time: the effect goes on in time.

The times occupied by the cause and by the effect succeed each other, the one ending at the point of time at which the other begins. But the time which the cause occupies is really composed of a series of instants of uniform motion interposed between instantaneous forces; and during the time that this series of causes is going on, to make up the aggregate cause, a series of effects is going on to make up the final effect. There is a progressive cause and a progressive effect which go on together, and occupy the same finite time; and this simultaneous progression is composed of all the simultaneous instantaneous steps of cause and effect. The aggregate cause is the sum of the progression of causes; the final effect is the last term of the progression of effects. At each step, as the Reviewer says, cause is transformed into effect; and it is treasured up in the results during the intermediate intervals; and the time occupied is not the time which intervenes between cause and effect at each step, but the time which intervenes between these transformations.

I have supposed forces to act at distinct instants, and to cease to act in the intervals between; and then, the aggregate of such intervals to make up a finite time, during which an aggregate force acts. But if the action of the force be rigorously continuous, it will easily be seen that all the consequences as to cause and effect will be the same; the discontinuous action being merely the usual artifice by which, in mathematical reasonings, we obtain results respecting continuous changes. It will still be true, that the uniform motion which takes place after a continuous force has acted, is the effect subsequent to the cause; while the change which takes place at any instant by the action of the force, is the instantaneous effect simultaneous with the cause.

It may be objected, that this solution does not appear immediately to apply: for the motion of a clock is not uniform during any portion of the time. The parts move by intervals of varied motion and of rest; or by oscillations backwards and forwards; and the succession of forces which acts during any oscillation, or any cycle of motion, is repeated during the succeeding oscillation or cycle, and so on indefinitely; and if an alteration be made in the parts, it is not a change once for all, but recurs in its operation in every cycle of the motion.

But it will be found that this circumstance does not prevent the same explanation from being still applicable with a slight

modification. Instead of uniform motion in the intervals of causation, we shall have to speak of *steady going*: and instead of considering all the forces which affect the motion as causes of change of uniform motion, we shall have to speak of changes in the parts of the mechanism as causes of *change of rate of going*. With this modification, it will still be true, that any instantaneous cause produces its instantaneous effect simultaneously, while the permanent effect is subsequent to the change which is its cause. The steady going of the clock is assumed as a normal condition, in which it measures the progress of time; and in this assumption, the notion of cause and effect is not brought into view. But a steady rate thus denoting the mean passage of time, a change in the rate indicates a cause of change. The *change of rate*, as an instantaneous transition from one rate to another, is *simultaneous* with the change in the parts. But then the *changed rate* as a continued condition in which, no new change supervening, the rate again measures the progress of time, is *subsequent* to the change of parts, for it begins when that ends, and continues when the progress of that has ceased.

If, however, this be a satisfactory solution of the difficulty in the case of mechanism, how shall we apply the same views to the other cases? Growth, the effect of food, is subsequent to the act of taking food; disorder, the effect of poison, is subsequent to the introduction of poison into the system. Can we say that the animal would continue unchanged if it were not to take food; and that food is the cause of a change, namely, of growth? This is manifestly false; for if the animal were not to take food, it would soon perish. But the analogy of the former case, of the clock, will enable us to avoid this perplexity. As we assumed a steady rate of going in the clock to be the measure of time when we considered the effect of mechanism, so we assume a steady rate of action in the animal functions to be the measure of the progress of time when we consider the causes which act upon the development and health of animals. Digestion, and of course nutrition, are a part of this normal condition; they are involved in the steady going of the animal mechanism, and we must suppose these functions to go regularly on, in order that the animal may preserve its character of animal. Food and digestion may be considered as causes of the continued existence of the animal, in the same way in which the form of the parts of a clock is the cause of the steady going of a clock. And when we

come to consider causes of change, this kind of causation, which produces a normal condition of things, merely measuring the flow of time, is left out of our account. We can conceive an uniform condition of animal existence, the animal neither growing nor wasting. This being taken as the normal condition, any deviation from this condition indicates a cause, and is taken as the evidence and measure of the cause of change. And thus, in a growing animal, the food partly keeps the animal in continued animal existence, and partly, and in addition to this, causes its growth. Food, in the former view, is always circulating in the system, and is supposed to be uniformly administered; the cycles of nutrition being merged in the notion of uniform existence, as the oscillations of the pendulum in a clock are merged in the notion of uniform going; and the elementary steps of nutrition which are, in this view, supposed to take place at each instant, produce their instantaneous effect, for they are requisite in the cycle of animal processes which goes on from instant to instant. But on the other hand, in considering growth, we compare the state of an animal with a preceding state, and consider the nutriment taken in the intervening time as the cause of the change: hence this nutriment, as an aggregate, is considered as the cause of growth of the animal; and in this view the effect is subsequent to the cause. But yet here, as in the case of mechanism, the progressive effect is simultaneous, step by step, with the progressive cause. There is a series of operations; as for instance, intussusception, digestion, assimilation, growth: each of these is a progressive operation; and in the progress of each operation, the steps of the effect and the instantaneous forces are simultaneous. But the end of one operation is the beginning of the next, or at least in part, and hence we have time occupied by the succession. The end of intussusception is the beginning of digestion, the end of digestion the beginning of assimilation, and so on. These aggregate effects succeed each other; and hence growth is subsequent to the taking of food; though each instantaneous force of animal life, no less than of mechanism, produces an effect simultaneous with its action. Each of these separate operations is an aggregate operation, and occupies time; and each aggregate effect is a condition of the action of the cause in the next operation.

Again; if an animal in a permanent condition, neither waxing nor wasting, may be taken as the normal state in which the

discussion of such questions would carry me too far, and would involve speculations not included within the acknowledged domain of this Society, from which I therefore abstain. But I may say, before quitting the subject, that I do not think the suppositions above suggested are untenable; and that in order to include moral causation under the maxims of causation in general, we must necessarily make some such hypothesis. The peculiarity of that kind of causation which the will and the character exert, and which is exerted upon the will and the character, would make this case far more complex and difficult than those already considered; but, at the same time, would offer us the means of explaining what may seem harsh, in the above analogy. For instance, we should have to assume such a maxim as this: that in moral causation, time is not to be measured by the flow of mechanical or physiological events;—not by the clock, or by the pulse. Moral causation has its own clock, its own pulse, in the progress of man's moral being; and by this measure of time is the relation of moral cause and effect to be defined.

That in estimating moral causation, the progress of time is necessarily estimated by moral changes, and not by machinery,—by the progress of events, and not by the going of the clock,—is a truth familiar as a practical maxim to all who give their thoughts to dramatic or narrative fictions. Who feels any thing incongruous or extravagantly hurried in the progress of events in that great exhibition of moral causation, the tragedy of Othello? If we were asked what time those vast and terrible and complex changes of the being and feelings of the characters occupy, we should say, that, measured on its own scale, the event is of great extent;—that the transaction is of considerable magnitude in all ways. But if, with previous critics, we look into the progress of time by the day and the hour—what is the measure of this history? Forty-eight hours.

But I am going beyond the boundaries of the speculations which we usually follow in this room, and will conclude.

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## ESSAY V.

### ON THE FUNDAMENTAL ANTITHESIS OF PHILOSOPHY\*.

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I HAVE upon former occasions laid before the Society dissertations on certain questions which may be termed metaphysical:—on the nature of the truth of the laws of motion:—on the question whether all matter is heavy:—and on the question whether cause and effect are successive or simultaneous. As these dissertations have not failed to excite some interest, I hope that I shall have the indulgence of the Society in making a few remarks on another question of the same kind. In doing this, as my object is to throw some light if possible on a matter of considerable obscurity and difficulty, I shall not attempt to avoid the occasional repetition of a sentence or two which I may have, in substance, delivered elsewhere.

1. All persons who have attended in any degree to the views generally current of the nature of reasoning are familiar with the distinction of necessary truths and truths of experience; and few such persons, or at least few students of mathematics, require to have this distinction explained or enforced. All geometricians are satisfied that the geometrical truths with which they are conversant are necessarily true: they not only *are* true, but they *must be* true. The meaning of the terms being understood, and the proof being gone through, the truth of the proposition must be assented to. That parallelograms upon the same base and between the same parallels are equal;—that angles in the same segment are equal;—these are propositions which we learn to be true by demonstrations deduced from definitions and axioms; and which, when we have thus learnt them, we see could not be otherwise. On the other hand, there are other truths which we learn from experience; as for instance, that the stars revolve round the pole in one day; and that the moon goes through her phases from full to full again in thirty days. These truths we see to be true; but we know them only by experience. Men never could have dis-

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covered them without looking at the stars and the moon ; and having so learnt them, still no one will pretend to say that they are necessarily true. For aught we can see, things might have been otherwise ; and if we had been placed in another part of the solar system, then, according to the opinions of astronomers, experience would have presented them otherwise.

2. I take the astronomical truths of experience to contrast with the geometrical necessary truths, as being both of a familiar definite sort ; we may easily find other examples of both kinds of truth. The truths which regard numbers are necessary truths. It is a necessary truth, that 27 and 38 are equal to 65 ; that half the sum of two numbers added to half their difference is equal to the greater number. On the other hand, that sugar will dissolve in water ; that plants cannot live without light ; and in short, the whole body of our knowledge in chemistry, physiology, and the other inductive sciences, consists of truths of experience. If there be any science which offer to us truths of an ambiguous kind, with regard to which we may for a moment doubt whether they are necessary or experiential, we will defer the consideration of them till we have marked the distinction of the two kinds more clearly.

3. One mode in which we may express the difference of necessary truths and truths of experience, is, that necessary truths are those of which we cannot distinctly conceive the contrary. We can very readily conceive the contrary of experiential truths. We can conceive the stars moving about the pole or across the sky in any kind of curves with any velocities ; we can conceive the moon always appearing during the whole month as a luminous disk, as she might do if her light were inherent and not borrowed. But we cannot conceive one of the parallelograms on the same base and between the same parallels larger than the other ; for we find that, if we attempt to do this, when we separate the parallelograms into parts, we have to conceive one triangle larger than another, both having all their parts equal ; which we cannot conceive at all, if we conceive the triangles distinctly. We make this impossibility more clear by conceiving the triangles to be placed so that two sides of the one coincide with two sides of the other ; and it is then seen, that in order to conceive the triangles unequal, we must conceive the two bases which have the same extremities both ways, to be different lines, though both

straight lines. This it is impossible to conceive: we assent to the impossibility as an axiom, when it is expressed by saying, that two straight lines cannot inclose a space; and thus we cannot distinctly conceive the contrary of the proposition just mentioned respecting parallelograms.

4. But it is necessary, in applying this distinction, to bear in mind the terms of it;—that we cannot *distinctly* conceive the contrary of a necessary truth. For in a certain loose, indistinct way, persons conceive the contrary of necessary geometrical truths, when they erroneously conceive false propositions to be true. Thus, Hobbes erroneously held that he had discovered a means of geometrically doubling the cube, as it is called, that is, finding two mean proportionals between two given lines; a problem which cannot be solved by plane geometry. Hobbes not only proposed a construction for this purpose, but obstinately maintained that it was right, when it had been proved to be wrong. But then, the discussion showed how indistinct the geometrical conceptions of Hobbes were; for when his critics had proved that one of the lines in his diagram would not meet the other in the point which his reasoning supposed, but in another point near to it; he maintained, in reply, that one of these points was large enough to include the other, so that they might be considered as the same point. Such a mode of conceiving the opposite of a geometrical truth, forms no exception to the assertion, that this opposite cannot be distinctly conceived.

5. In like manner, the indistinct conceptions of children and of rude savages do not invalidate the distinction of necessary and experiential truths. Children and savages make mistakes even with regard to numbers; and might easily happen to assert that 27 and 38 are equal to 63 or 64. But such mistakes cannot make such arithmetical truths cease to be necessary truths. When any person conceives these numbers and their addition distinctly, by resolving them into parts, or in any other way, he sees that their sum is necessarily 65. If, on the ground of the possibility of children and savages conceiving something different, it be held that this is not a necessary truth, it must be held on the same ground, that it is not a necessary truth that 7 and 4 are equal to 11; for children and savages might be found so unfamiliar with numbers as not to reject the assertion that 7 and 4 are 10, or even that 4 and 3 are 6, or 8.



But I suppose that no persons would on such grounds hold that these arithmetical truths are truths known only by experience.

6. Necessary truths are established, as has already been said, by demonstration, proceeding from definitions and axioms, according to exact and rigorous inferences of reason. Truths of experience are collected from what we see, also according to inferences of reason, but proceeding in a less exact and rigorous mode of proof. The former depend upon the relations of the ideas which we have in our minds: the latter depend upon the appearances or phenomena, which present themselves to our senses. Necessary truths are formed from our thoughts, the elements of the world within us; experiential truths are collected from things, the elements of the world without us. The truths of experience, as they appear to us in the external world, we call Facts; and when we are able to find among our ideas a train which will conform themselves to the apparent facts, we call this a Theory.

7. This distinction and opposition, thus expressed in various forms; as Necessary and Experiential Truth, Ideas and Senses, Thoughts and Things, Theory and Fact, may be termed the *Fundamental Antithesis of Philosophy*; for almost all the discussions of philosophers have been employed in asserting or denying, explaining or obscuring this antithesis. It may be expressed in many other ways; but is not difficult, under all these different forms, to recognize the same opposition: and the same remarks apply to it under its various forms, with corresponding modifications. Thus, as we have already seen, the antithesis agrees with that of Reasoning and Observation: again, it is identical with the opposition of Reflection and Sensation: again, sensation deals with Objects; facts involve Objects, and generally all things without us are Objects:—Objects of sensation, of observation. On the other hand, we ourselves who thus observe objects, and in whom sensation is, may be called the Subjects of sensation and observation. And this distinction of Subject and Object is one of the most general ways of expressing the fundamental antithesis, although not yet perhaps quite familiar in English. I shall not scruple however to speak of the Subjective and Objective element of this antithesis, where the expressions are convenient.

8. All these forms of antithesis, and the familiar references to them which men make in all discussions, shew the fundamental

and necessary character of the antithesis. We can have no knowledge without the union, no philosophy without the separation, of the two elements. We can have no knowledge, except we have both impressions on our senses from the world without, and thoughts from our minds within:—except we attend to things, and to our ideas;—except we are passive to receive impressions, and active to compare, combine, and mould them. But on the other hand, philosophy seeks to distinguish the impressions of our senses from the thoughts of our minds;—to point out the difference of ideas and things;—to separate the active from the passive faculties of our being. The two elements, sensations and ideas, are both requisite to the existence of our knowledge, as both matter and form are requisite to the existence of a body. But philosophy considers the matter and the form separately. The properties of the form are the subject of geometry, the properties of the matter are the subject of chemistry or mechanics.

9. But though philosophy considers these elements of knowledge separately, they cannot really be separated, any more than can matter and form. We cannot exhibit matter without form, or form without matter; and just as little can we exhibit sensations without ideas, or ideas without sensations;—the passive or the active faculties of the mind detached from each other.

In every act of my knowledge, there must be concerned the things whereof I know, and thoughts of me who know: I must both passively receive or have received impressions, and I must actively combine them and reason on them. No apprehension of things is purely ideal: no experience of external things is purely sensational. If they be conceived as *things*, the mind must have been awoke to the conviction of things by sensation: if they be *conceived* as things, the expressions of the senses must have been bound together by conceptions. If we *think* of any *thing*, we must recognize the existence both of thoughts and of things. *The fundamental antithesis of philosophy is an antithesis of inseparable elements.*

10. Not only cannot these elements be separately exhibited, but they cannot be separately conceived and described. The description of them must always imply their relation; and the names by which they are denoted will consequently always bear a relative significance. And thus *the terms which denote the*

*fundamental antithesis of philosophy cannot be applied absolutely and exclusively in any case.* We may illustrate this by a consideration of some of the common modes of expressing the antithesis of which we speak. The terms Theory and Fact are often emphatically used as opposed to each other: and they are rightly so used. But yet it is impossible to say absolutely in any case, This is a Fact and not a Theory; this is a Theory and not a Fact, meaning by Theory, true Theory. Is it a fact or a theory that the stars appear to revolve round the pole? Is it a fact or a theory that the earth is a globe revolving round its axis? Is it a fact or a theory that the earth revolves round the sun? Is it a fact or a theory that the sun attracts the earth? Is it a fact or a theory that a loadstone attracts a needle? In all these cases, some persons would answer one way and some persons another. A person who has never watched the stars, and has only seen them from time to time, considers their circular motion round the pole as a theory, just as he considers the motion of the sun in the ecliptic as a theory, or the apparent motion of the inferior planets round the sun in the zodiac. A person who has compared the measures of different parts of the earth, and who knows that these measures cannot be conceived distinctly without supposing the earth a globe, considers its globular form a fact, just as much as the square form of his chamber. A person to whom the grounds of believing the earth to revolve round its axis and round the sun, are as familiar as the grounds for believing the movements of the mail-coaches in this country, conceives the former events to be facts, just as steadily as the latter. And a person who, believing the fact of the earth's annual motion, refers it distinctly to its mechanical course, conceives the sun's attraction as a fact, just as he conceives as a fact the action of the wind which turns the sails of a mill. We see then, that in these cases we cannot apply absolutely and exclusively either of the terms, Fact or Theory. Theory and Fact are the elements which correspond to our Ideas and our Senses. The Facts are Facts so far as the Ideas have been combined with the sensations and absorbed in them: the Theories are Theories so far as the Ideas are kept distinct from the sensations, and so far as it is considered as still a question whether they can be made to agree with them. A true Theory is a fact, a Fact is a familiar theory.

In like manner, if we take the terms Reasoning and Ob-

ervation; at first sight they appear to be very distinct. Our observation of the world without us, our reasonings in our own minds, appear to be clearly separated and opposed. But yet we shall find that we cannot apply these terms absolutely and exclusively. I see a book lying a few feet from me: is this a matter of observation? At first, perhaps, we might be inclined to say that it clearly is so. But yet, all of us, who have paid any attention to the process of vision, and to the mode in which we are enabled to judge of the distance of objects, and to judge them to be distant objects at all, know that this judgment involves inferences drawn from various sensations;—from the impressions on our two eyes;—from our muscular sensations; and the like. These inferences are of the nature of reasoning, as much as when we judge of the distance of an object on the other side of a river by looking at it from different points, and stepping the distance between them. Or again: we observe the setting sun illuminate a gilded weathercock; but this is as much a matter of reasoning as when we observe the phases of the moon, and infer that she is illuminated by the sun. All observation involves inferences, and inference is reasoning.

11. Even the simplest terms by which the antithesis is expressed cannot be separated: ideas and sensations, thoughts and things, subject and object, cannot in any case be applied absolutely and exclusively. Our sensations require ideas to bind them together, namely, ideas of space, time, number, and the like. If not so bound together, sensations do not give us any apprehension of things or objects. All things, all objects, must exist in space and in time—must be one or many. Now space, time, number, are not sensations or things. They are something different from, and opposed to sensations and things. We have termed them ideas. It may be said they are *relations* of things, or of sensations. But granting this form of expression, still a *relation* is not a thing or a sensation; and therefore we must still have another and opposite element, along with our sensations. And yet, though we have thus these two elements in every act of perception, we cannot designate any portion of the act as absolutely and exclusively belonging to one of the elements. Perception involves sensation, along with ideas of time, space, and the like; or, if any one prefers the expression, we may say, Perception involves sensations along with the apprehension of relations. Perception is sensation, along with

such ideas as make sensation into an apprehension of things or objects.

12. And as perception of objects implies ideas,—as observation implies reasoning;—so, on the other hand, ideas cannot exist where sensation has not been: reasoning cannot go on when there has not been previous observation. This is evident from the necessary order of development of the human faculties. Sensation necessarily exists from the first moments of our existence, and is constantly at work. Observation begins before we can suppose the existence of any reasoning which is not involved in observation. Hence, at whatever period we consider our ideas, we must consider them as having been already engaged in connecting our sensations, and as having been modified by this employment. By being so employed, our ideas are unfolded and defined; and such development and definition cannot be separated from the ideas themselves. We cannot conceive space without boundaries or forms; now forms involve sensations. We cannot conceive time without events which mark the course of time; but events involve sensations. We cannot conceive number without conceiving things which are numbered; and things imply sensations. And the forms, things, events, which are thus implied in our ideas, having been the objects of sensation constantly in every part of our life, have modified, unfolded and fixed our ideas, to an extent which we cannot estimate, but which we must suppose to be essential to the processes which at present go on in our minds. We cannot say that objects create ideas; for to perceive objects we must already have ideas. But we may say, that objects and the constant perception of objects have so far modified our ideas, that we cannot, even in thought, separate our ideas from the perception of objects.

We cannot say of any ideas, as of the idea of space, or time, or number, that they are absolutely and exclusively ideas. We cannot conceive what space, or time, or number would be in our minds, if we had never perceived any thing or things in space or time. We cannot conceive ourselves in such a condition as never to have perceived any thing or things in space or time. But, on the other hand, just as little can we conceive ourselves becoming acquainted with space and time or numbers as objects of sensation. We cannot reason without having the operations of our minds affected by previous sensations; but we

cannot conceive reasoning to be merely a series of sensations. In order to be used in reasoning, sensation must become observation; and, as we have seen, observation already involves Reasoning. In order to be connected by our ideas, sensations must be Things or objects, and things or objects already include ideas. And thus none of the terms by which the fundamental antithesis is expressed can be absolutely and exclusively applied.

13. I will make a remark suggested by the views which have thus been presented. Since, as we have just seen, none of the terms which express the fundamental antithesis can be applied absolutely and exclusively, the absolute application of the antithesis in any particular case can never be a conclusive or immoveable principle. This remark is the more necessary to be borne in mind, as the terms of this antithesis are often used in a vehement and peremptory manner. Thus we are often told that such a thing is a *Fact* and not a *Theory*, with all the emphasis which, in speaking or writing, tone or italics or capitals can give. We see from what has been said, that when this is urged, before we can estimate the truth, or the value of the assertion, we must ask to whom is it a fact? what habits of thought, what previous information, what Ideas does it imply, to conceive the fact as a fact? Does not the apprehension of the fact imply assumptions which may with equal justice be called theory, and which are perhaps false theory? in which case, the fact is no fact. Did not the ancients assert it as a Fact, that the earth stood still, and the stars moved? and can any Fact have stronger apparent evidence to justify persons in asserting it emphatically than this had? These remarks are by no means urged in order to shew that no Fact can be certainly known to be true; but only to shew that no Fact can be certainly shown to be a Fact merely by calling it a Fact, however emphatically. There is by no means any ground of general skepticism with regard to truth involved in the doctrine of the necessary combination of two elements in all our knowledge. On the contrary, Ideas are requisite to the essence, and Things to the reality of our knowledge in every case. The proportions of Geometry and Arithmetic are examples of knowledge respecting our Ideas of space and number, with regard to which there is no room for doubt. The doctrines of Astronomy are examples of truths not less certain respecting the Facts of the external world.

14. I remark further, that since in every act of knowledge, observation or perception, both the elements of the fundamental antithesis are involved, and involved in a manner inseparable even in our conceptions, it must always be possible to derive one of these elements from the other, if we are satisfied to accept, as proof of such derivation, that one always co-exists with and implies the other. Thus an opponent may say, that our ideas of space, time, and number, are derived from our sensations or perceptions, because we never were in a condition in which we had the ideas of space and time, and had not sensations or perceptions. But then, we may reply to this, that we no sooner perceive objects than we perceive them as existing in space and time, and therefore the ideas of space and time are not derived from the perceptions. In the same manner, an opponent may say, that all knowledge which is involved in our reasonings is the result of experience; for instance, our knowledge of geometry. For every geometrical principle is presented to us by experience as true; beginning with the simplest, from which all others are derived by processes of exact reasoning. But to this we reply, that experience cannot be the origin of such knowledge; for though experience shows that such principles are true, it cannot show that they *must be* true, which we also know. We never have seen, as a matter of observation, two straight lines inclosing a space; but we venture to say further, without the smallest hesitation, that we never shall see it; and if any one were to tell us that, according to his experience, such a form was often seen, we should only suppose that he did not know what he was talking of. No number of acts of experience can add to the certainty of our knowledge in this respect; which shows that our knowledge is not made up of acts of experience. We cannot test such knowledge by experience; for if we were to try to do so, we must first know that the lines with which we make the trial *are* straight; and we have no test of straightness better than this, that two such lines cannot inclose a space. Since then, experience can neither destroy, add to, nor test our axiomatic knowledge, such knowledge cannot be derived from experience. Since no one act of experience can affect our knowledge, no numbers of acts of experience can make it.

15. To this a reply has been offered, that it is a characteristic property of geometric forms that the ideas of them exactly resemble the sensations; so that these ideas are as fit

subjects of experimentation as the realities themselves; and that by such experimentation we learn the truth of the axioms of geometry. I might very reasonably ask those who use this language to explain how a particular class of ideas can be said to resemble sensations; how, if they do, we can know it to be so; how we can prove this resemblance to belong to geometrical ideas and sensations; and how it comes to be an especial characteristic of those. But I will put the argument in another way. Experiment can only show what is, not what must be. If experimentation on ideas shows what must be, it is different from what is commonly called experience.

I may add, that not only the mere use of our senses cannot show that the axioms of geometry *must be* true, but that, without the light of our ideas, it cannot even show that they *are* true. If we had a segment of a circle a mile long and an inch wide, we should have two lines inclosing a space; but we could not, by seeing or touching any part of either of them, discover that it was a bent line.

16. That mathematical truths are not derived from experience is perhaps still more evident, if greater evidence be possible, in the case of numbers. We assert that 7 and 8 are 15. We find it so, if we try with counters, or in any other way. But we do not, on that account, say that the knowledge is derived from experience. We refer to our conceptions of seven, of eight, and of addition, and as soon as we possess these conceptions distinctly, we see that the sum must be fifteen. We cannot be said to make a trial, for we should not believe the apparent result of the trial if it were different. If any one were to say that the multiplication table is a table of the results of experience, we should know that he could not be able to go along with us in our researches into the foundations of human knowledge; nor, indeed, to pursue with success any speculations on the subject.

17. Attempts have also been made to explain the origin of axiomatic truths by referring them to the "association of ideas." But this is one of the cases in which the word *association* has been applied so widely and loosely, that no sense can be attached to it. Those who have written with any degree of distinctness on the subject, have truly taught, that the habitual association of the Ideas leads us to believe a connexion of the Things: but they have never told us that this association gave us the power



of forming the ideas. Association may determine belief, but it cannot determine the possibility of our conceptions. The African king did not believe that water could become solid, because he had never seen it in that state. But that accident did not make it impossible to conceive it so, any more than it is impossible for us to conceive frozen quicksilver, or melted diamond, or liquefied air; which we may never have seen, but have no difficulty in conceiving. If there were a tropical philosopher really incapable of conceiving water solidified, he must have been brought into that mental condition by abstruse speculations on the necessary relations of solidity and fluidity, not by the association of ideas.

18. To return to the results of the nature of the Fundamental Antithesis. As by assuming universal and indissoluble connexion of ideas with perceptions, of knowledge with experience, as an evidence of derivation, we may assert the former to be derived from the latter, so might we, on the same ground, assert the latter to be derived from the former. We see all forms in space; and we might hence assert all forms to be mere modifications of our idea of space. We see all events happen in time; and we might hence assert all events to be merely limitations and boundary-marks of our idea of time. We conceive all collections of things as two or three, or some other number: it might hence be asserted that we have an original idea of number, which is reflected in external things. In this case, as in the other, we are met at once by the impossibility of this being a complete account of our knowledge. Our ideas of space, of time, of number, however distinctly reflected to us with limitations and modifications, must be reflected, limited and modified by something different from themselves. We must have visible or tangible forms to limit space, perceived events to mark time, distinguishable objects to exemplify number. But still, in forms, and events, and objects, we have a knowledge which they themselves cannot give us. For we know, without attending to them, that whatever they are, they will conform and must conform to the truths of geometry and arithmetic. There is an ideal portion in all our knowledge of the external world; and if we were resolved to reduce all our knowledge to one of its two antithetical elements, we might say that all our knowledge consists in the relation of our ideas. Wherever there is necessary truth, there must be something more than sensation can supply: and the

necessary truths of geometry and arithmetic show us that our knowledge of objects in space and time depends upon necessary relations of ideas, whatever other element it may involve.

19. This remark may be carried much further than the domain of geometry and arithmetic. Our knowledge of matter may at first sight appear to be altogether derived from the senses. Yet we cannot derive from the senses our knowledge of a truth which we accept as universally certain;—namely, that we cannot by any process add to or diminish the quantity of matter in the world. This truth neither is nor can be derived from experience; for the experiments which we make to verify it pre-suppose its truth. When the philosopher was asked what was the weight of smoke, he bade the inquirer subtract the weight of the ashes from the weight of the fuel. Every one who thinks clearly of the changes which take place in matter, assents to the justice of this reply: and this, not because any one had found by trial that such was the weight of the smoke produced in combustion, but because the weight lost was assumed to have gone into some other form of matter, not to have been destroyed. When men began to use the balance in chemical analysis, they did not prove by trial, but took for granted, as self-evident, that the weight of the whole must be found in the aggregate weight of the elements. Thus it is involved in the idea of matter that its amount continues unchanged in all changes which takes places in its consistence. This is a necessary truth: and thus our knowledge of matter, as collected from chemical experiments, is also a modification of our idea of matter as the material of the world incapable of addition or diminution.

20. A similar remark may be made with regard to the mechanical properties of matter. Our knowledge of these is reduced, in our reasonings, to principles which we call the laws of motion. These laws of motion, as I have endeavoured to shew in a paper already printed by the Society, depend upon the idea of Cause, and involve necessary truths, which are necessarily implied in the idea of cause;—namely, that every change of motion must have a cause—that the effect is measured by the cause;—that re-action is equal and opposite to action. These principles are not derived from experience. No one, I suppose, would derive from experience the principle, that every event must have a cause. Every attempt to see the traces of cause in the world assumes this principle. I do not say that these

principles are anterior to experience; for I have already, I hope, shewn, that neither of the two elements of our knowledge is, or can be, anterior to the other. But the two elements are co-ordinate in the development of the human mind; and the ideal element may be said to be the origin of our knowledge with the more propriety of the two, inasmuch as our knowledge is the relation of ideas. The other element of knowledge, in which sensation is concerned, and which embodies, limits, and defines the necessary truths which express the relations of our ideas, may be properly termed Experience; and I have, in the Memoir just quoted, endeavoured to shew how the Principles concerning mechanical causation, which I have just stated, are, by observation and experiment, limited and defined, so that they become the Laws of Motion. And thus we see that such knowledge is derived from ideas, in a sense quite as general and rigorous, to say the least, as that in which it is derived from experience.

21. I will take another example of this; although it is one less familiar, and the consideration of it perhaps a little more difficult and obscure. The objects which we find in the world, for instance, minerals and plants, are of different kinds; and according to their kinds, they are called by various names, by means of which we know what we mean when we speak of them. The discrimination of these kinds of objects, according to their different forms and other properties, is the business of chemistry and botany. And this business of discrimination, and of consequent classification, has been carried on from the first periods of the development of the human mind, by an industrious and comprehensive series of observations and experiments; the only way in which any portion of the task could have been effected. But as the foundation of all this labour, and as a necessary assumption during every part of its progress, there has been in men's minds the principle, that objects are so distinguishable by resemblances and differences, that they may be named, and known by their names. This principle is involved in the idea of a Name; and without it no progress could have been made. The principle may be briefly stated thus:—Intelligible Names of Kinds are possible. If we suppose this not to be so, language can no longer exist, nor could the business of human life go on. If instead of having certain definit kinds of minerals, gold, iron, copper, and the like, of which the external forms and characters

are constantly connected with the same properties and qualities, there were no connexion between the appearance and the properties of the object ;—if what seemed externally iron might turn out to resemble lead in its hardness ; and what seemed to be gold during many trials, might at the next trial be found to be like copper ; not only all the uses of these minerals would fail, but they would not be distinguishable kinds of things, and the names would be unmeaning. And if this entire uncertainty as to kind and properties prevailed for all objects, the world would no longer be a world to which language was applicable. To man, thus unable to distinguish objects into kinds, and call them by names, all knowledge would be impossible, and all definite apprehension of external objects would fade away into an inconceivable confusion. In the very apprehension of objects as intelligibly sorted, there is involved a principle which springs within us, contemporaneous, in its efficacy, with our first intelligent perception of the kinds of things of which the world consists. We assume, as a necessary basis of our knowledge, that things are of definite kinds ; and the aim of chemistry, botany, and other sciences is, to find marks of these kinds ; and along with these, to learn their definitely-distinguished properties. Even here, therefore, where so large a portion of our knowledge comes from experience and observation, we cannot proceed without a necessary truth derived from our ideas, as our fundamental principle of knowledge.

22. What the Marks are, which distinguish the constant differences of Kinds of things (definite marks, selected from among many unessential appearances), and what their definite properties are, when they are so distinguished, are parts of our knowledge to be learnt from observation, by various processes ; for instance, among others, by chemical analysis. We find the differences of bodies, as shown by such analysis, to be of this nature :—that there are various elementary bodies, which, combining in different definite proportions, form kinds of bodies definitely different. But, in arriving at this conclusion, we introduce a new idea, that of Elementary Composition, which is not extracted from the phenomena, but supplied by the mind, and introduced in order to make the phenomena intelligible. That this notion of elementary composition is not supplied by the chemical phenomena of combustion, mixture, &c. as merely an observed fact, we see from this ; that men had in ancient

times performed many experiments in which elementary composition was concerned, and had not seen the fact. It never was truly seen till modern times ; and when seen, it gave a new aspect to the whole body of known facts. This Idea of Elementary Composition, then, is supplied by the mind, in order to make the facts of chemical analysis and synthesis intelligible *as* analysis and synthesis. And this idea being so supplied, there enters into our knowledge along with it a corresponding necessary principle ;—That the elementary composition of a body determines its kind and properties. This is, I say, a principle assumed, as a consequence of the idea of composition, not a result of experience ; for when bodies have been divided into their kinds, we take for granted that the analysis of a single specimen may serve to determine the analysis of all bodies of the same kind : and without this assumption, chemical knowledge with regard to the kinds of bodies would not be possible. It has been said that we take only one experiment to determine the composition of any particular kind of body, because we have a thousand experiments to determine that bodies of the same kind have the same composition. But this is not so. Our belief in the principle that bodies of the same kind have the same composition is not established by experiments, but is assumed as a necessary consequence of the ideas of Kind and of Composition. If, in our experiments, we found that bodies supposed to be of the same kind had not the same composition, we should not at all doubt of the principle just stated, but conclude at once that the bodies were *not* of the same kind ;—that the marks by which the kinds are distinguished had been wrongly stated. This is what has very frequently happened in the course of the investigations of chemists and mineralogists. And thus we have it, not as an experiential fact, but as a necessary principle of chemical philosophy, that the Elementary Composition of a body determines its Kind and Properties.

23. How bodies differ in their elementary composition, experiment must teach us, as we have already said that experiment has taught us. But as we have also said, whatever be the nature of this difference, Kinds must be definite, in order that Language may be possible : and hence, whatever be the terms in which we are taught by experiment to express the elementary composition of bodies, the result must be conformable to this principle, That the Differences of elementary composition are

definite. The law to which we are led by experiment is, that the elements of bodies continue in definite proportions according to weight. Experiments add other laws; as for instance, that of multiple proportions in different kinds of bodies composed of the same elements; but of these we do not here speak.

24. We are thus led to see that in our knowledge of mechanics, chemistry, and the like, there are involved certain necessary principles, derived from our ideas, and not from experience. But to this it may be objected, that the parts of our knowledge in which these principles are involved has, in historical fact, all been acquired by experience. The Laws of Motion, the Doctrine of Definite Proportions, and the like, have all become known by experiment and observation; and so far from being seen as necessary truths, have been discovered by long-continued labours and trials, and through innumerable vicissitudes of confusion, error, and imperfect truth. This is perfectly true: but does not at all disprove what has been said. Perception of external objects and experience, experiment and observation, are needed, not only, as we have said, to supply the objective element of all knowledge—to embody, limit, define, and modify our ideas; but this intercourse with objects is also requisite to unfold and fix our ideas themselves. As we have already said, ideas and facts can never be separated. Our ideas cannot be exercised and developed in any other form than in their combination with facts; and therefore the trials, corrections, controversies, by which the Matter of our knowledge is collected, is also the only way in which the Form of it can be rightly fashioned. Experience is requisite to the clearness and distinctness of our ideas, not because they are derived from experience, but because they can only be exercised upon experience. And this consideration sufficiently explains how it is that experiment and observation have been the means, and the only means, by which men have been led to a knowledge of the laws of nature. In reality, however, the necessary principles which flow from our ideas, and which are the basis of such knowledge, have not only been inevitably assumed in the course of such investigations, but have been often expressly promulgated in words by clear-minded philosophers, long before their true interpretation was assigned by experiment. This has happened with regard to such principles as those above mentioned; That every event must have a cause; That reaction is equal and opposite to action; That

the quantity of matter in the world cannot be increased or diminished: and there would be no difficulty in finding similar enunciations of the other principles above mentioned;—That the kinds of things have definite differences, and that these differences depend upon their elementary composition. In general, however, it may be allowed, that the necessary principles which are involved in those laws of nature of which we have a knowledge become then only clearly known, when the laws of nature are discovered which thus involve the necessary ideal element.

25. But since this is allowed, it may be further asked, how we are to distinguish between the necessary principle which is derived from our ideas, and the law of nature which is learnt by experience. And to this we reply, that the necessary principle may be known by the condition which we have already mentioned as belonging to such principles:—that it is impossible distinctly to conceive the contrary. We cannot conceive an event without a cause, except we abandon all distinct idea of cause; we cannot distinctly conceive two straight lines inclosing space; and if we seem to conceive this, it is only because we conceive indistinctly. We cannot conceive 5 and 3 making 7 or 9; if a person were to say that he could conceive this, we should know that he was a person of immature or rude or bewildered ideas, whose conceptions had no distinctness. And thus we may take it as the mark of a necessary truth, that we cannot conceive the contrary distinctly.

26. If it be asked what is the test of distinct conception (since it is upon the distinctness of conception that the matter depends), we may consider what answer we should give to this question if it were asked with regard to the truths of geometry. If we doubted whether any one had these distinct conceptions which enable him to see the necessary nature of geometrical truth, we should inquire if he could understand the axioms as axioms, and could follow, as demonstrative, the reasonings which are founded upon them. If this were so, we should be ready to pronounce that he had distinct ideas of space, in the sense now supposed. And the same answer may be given in any other case. That reasoner has distinct conceptions of mechanical causes who can see the axioms of mechanics as axioms, and can follow the demonstrations derived from them as demonstrations. If it be said that the science, as presented to him, may be erroneously constructed; that the axioms may not be axioms,

and therefore the demonstrations may be futile, we still reply, that the same might be said with regard to geometry: and yet that the possibility of this does not lead us to doubt either of the truth or of the necessary nature of the propositions contained in Euclid's Elements. We may add further, that although, no doubt, the authors of elementary books may be persons of confused minds, who present as axioms what are not axiomatic truths; yet that in general, what is presented as an axiom by a thoughtful man, though it may include some false interpretation or application of our ideas, will also generally include some principle which really is necessarily true, and which would still be involved in the axiom, if it were corrected so as to be true instead of false. And thus we still say, that if in any department of science a man can conceive distinctly at all, there are principles the contrary of which he cannot distinctly conceive, and which are therefore necessary truths.

27. But on this it may be asked, whether truth can thus depend upon the particular state of mind of the person who contemplates it; and whether that can be a necessary truth which is not so to all men. And to this we again reply, by referring to geometry and arithmetic. It is plain that truths may be necessary truths which are not so to all men, when we include men of confused and perplexed intellects; for to such men it is not a necessary truth that two straight lines cannot inclose a space, or that 14 and 17 are 31. It need not be wondered at, therefore, if to such men it does not appear a necessary truth that reaction is equal and opposite to action, or that the quantity of matter in the world cannot be increased or diminished. And this view of knowledge and truth does not make it depend upon the state of mind of the student, any more than geometrical knowledge and geometrical truth, by the confession of all, depend upon that state. We know that a man cannot have any knowledge of geometry without so much of attention to the matter of the science, and so much of care in the management of his own thoughts, as is requisite to keep his ideas distinct and clear. But we do not, on that account, think of maintaining that geometrical truth depends merely upon the state of the student's mind. We conceive that he knows it because it is true, not that it is true because he knows it. We are not surprized that attention and care and repeated thought should be requisite to the clear apprehension of truth. For such care and such repetition are



requisite to the distinctness and clearness of our ideas: and yet the relations of these ideas, and their consequences, are not produced by the efforts of attention or repetition which we exert. They are in themselves something which we may discover, but cannot make or change. The idea of space, for instance, which is the basis of geometry, cannot give rise to any doubtful propositions. What is inconsistent with the idea of space cannot be truly obtained from our ideas by any efforts of thought or curiosity; if we blunder into any conclusion inconsistent with the idea of space, our knowledge, so far as this goes, is no knowledge; any more than our observation of the external world would be knowledge, if, from haste or inattention, or imperfection of sense, we were to mistake the object which we see before us.

28. But further: not only has truth this reality, (which makes it independent of our mistakes,) that it must be what is really consistent with our ideas; but also, a further reality, to which the term is more obviously applicable, arising from the principle already explained, that ideas and perceptions are inseparable. For since, when we contemplate our ideas, they have been frequently embodied and exemplified in objects, and thus have been fixed and modified; and since this compound aspect is that under which we constantly have them before us, and free from which they cannot be exhibited; our attempts to make our ideas clear and distinct will constantly lead us to contemplate them as they are manifested in those external forms in which they are involved. Thus in studying geometrical truth, we shall be led to contemplate it as exhibited in visible and tangible figures;—not as if these could be sources of truth, but as enabling us more readily to compare the aspects which our ideas, applied to the world of objects, may assume. And thus we have an additional indication of the reality of geometrical truth, in the necessary possibility of its being capable of being exhibited in a visible or tangible form. And yet even this test by no means supersedes the necessity of distinct ideas, in order to a knowledge of geometrical truth. For in the case of the duplication of the cube by Hobbes, mentioned above, the diagram which he drew made two points appear to coincide, which did not really, and by the nature of our idea of space, coincide; and thus confirmed him in his error.

*Thus the inseparable nature of the Fundamental Antithesis of*

*Ideas and Things gives reality to our knowledge, and makes objective reality a corrective of our subjective imperfections in the pursuit of knowledge. But this objective exhibition of knowledge can by no means supersede a complete development of the subjective condition, namely, distinctness of ideas. And that there is a subjective condition, by no means makes knowledge altogether subjective, and thus deprives it of reality; because, as we have said, the subjective and the objective elements are inseparably bound together in the fundamental antithesis.*

29. It would be easy to apply these remarks to other cases, for instance, to the case of the principle we have just mentioned, that the differences of elementary composition of different kinds of bodies must be definite. We have stated that this principle is necessarily true;—that the contrary proposition cannot be distinctly conceived. But by whom? Evidently, according to the preceding reasoning, by a person who distinctly conceives Kinds, as marked by intelligible names, and Composition, as determining the kinds of bodies. Persons new to chemical and classificatory science may not possess these ideas distinctly; or rather, cannot possess them distinctly; and therefore cannot apprehend the impossibility of conceiving the opposite of the above principle; just as the schoolboy cannot apprehend the impossibility of the numbers in his multiplication table being other than they are. But this inaptitude to conceive, in either case, does not alter the necessary character of the truth: although, in one case, the truth is obvious to all except schoolboys and the like, and the other is probably not clear to any except those who have attentively studied the philosophy of elementary compositions. At the same time, this difference of apprehension of the truth in different persons does not make the truth doubtful or dependent upon personal qualifications; for in proportion as persons attain to distinct ideas, they will see the truth; and cannot, with such ideas, see anything as truth which is not truth. When the relations of elements in a compound become as familiar to a person as the relations of factors in a multiplication table, he will then see what are the necessary axioms of chemistry, as he now sees the necessary axioms of arithmetic.

30. There is also one other remark which I will here make. In the progress of science, both the elements of our knowledge are constantly expanded and augmented. By the exercise of observation and experiment, we have a perpetual accumulation

of facts, the materials of knowledge, the objective element. By thought and discussion, we have a perpetual development of man's ideas going on: theories are framed, the materials of knowledge are shaped into form; the subjective element is evolved; and by the necessary coincidence of the objective and subjective elements, the matter and the form, the theory and the facts, each of these processes furthers and corrects the other: each element moulds and unfolds the other. Now it follows, from this constant development of the ideal portion of our knowledge, that we shall constantly be brought in view of new Necessary Principles, the expression of the conditions belonging to the Ideas which enter into our expanding knowledge. These principles, at first dimly seen and hesitatingly asserted, at last become clearly and plainly self-evident. Such is the case with the principles which are the basis of the laws of motion. Such may soon be the case with the principles which are the basis of the philosophy of chemistry. Such may hereafter be the case with the principles which are to be the basis of the philosophy of the connected and related polarities of chemistry, electricity, galvanism, magnetism. That knowledge is possible in these cases, we know; that our knowledge may be reduced to principles gradually more simple, we also know; that we have reached the last stage of simplicity of our principles, few cultivators of the subject will be disposed to maintain; and that the additional steps which lead toward very simple and general principles will also lead to principles which recommend themselves by a kind of axiomatic character, those who judge from the analogy of the past history of science will hardly doubt. That the principles thus axiomatic in their form, do also express some relation of our ideas, of which experiment and observation have given the true and real interpretation, is the doctrine which I have here attempted to establish and illustrate in the most clear and undoubted of the existing sciences; and the evidence of this doctrine in those cases seems to be unexceptionable, and to leave no room to doubt that such is the universal type of the progress of science. Such a doctrine, as we have now seen, is closely connected with the views here presented of the nature of the Fundamental Antithesis of Philosophy, which I have endeavoured to illustrate.

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## ESSAY VI.

### REMARKS ON A REVIEW OF THE PHILOSOPHY OF THE INDUCTIVE SCIENCES\*.

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*Trinity Lodge, April 11th, 1844.*

MY DEAR HERSCHEL,

BEING about to send you a copy of a paper on a philosophical question just printed in the Transactions of our Cambridge Society, I am tempted to add, as a private communication, a few Remarks on another aspect of the same question. These Remarks I think I may properly address to you. They will refer to an Article in the "Quarterly Review" for June 1841, respecting my "History" and "Philosophy" of the Inductive Sciences; and without assigning any other reason, I may say that the interest I know you to take in speculations on such subjects makes me confident that you will give a reasonable attention to what I may have to say on the subject of that Article. With the Reviewal itself, I am so far from having any quarrel, that when it appeared I received it as affording all that I hoped from Public Criticism. The degree and the kind of admiration bestowed upon my works by a writer so familiar with science, so comprehensive in his views, and so equitable in his decisions, as the Reviewer manifestly was, I accepted as giving my work a stamp of acknowledged value which few other hands could have bestowed.

You may perhaps recollect, however, that the Reviewer dissented altogether from some of the general views which I had maintained, and especially from a general view which is also, in the main, that presented in the preceding Essay, namely, that, besides Facts, Ideas are an indispensable source of our knowledge; that Ideas are the ground of necessary truth; that the Idea of Space, in particular, is the ground of the necessary truths of geometry. This question, and especially as limited to the last form, will be the subject of my Remarks in the first place; and I wish to consider the Reviewer's objections with the respect which their subtlety and depth of thought well deserve.

The Reviewer makes objections to the account which I have

\* *A Letter to Sir John F. W. Herschel, Bart.*

given of the source whence geometrical truth derives its characters of being necessary and universal; but he is not one of those metaphysicians who deny those characters to the truths of geometry. He allows in the most ample manner that the truths of geometry *are* necessary. The question between us therefore is, From what this character is derived? The Reviewer prefers, indeed, to have it considered that the question is not concerning the necessity, but, as he says, the universality of these truths; or rather, the nature and grounds of our conviction of their universality. He might have said, with equal justice, the nature and grounds of our conviction of their necessity. For his objection to the term *necessity* in this case—"that all the propositions about realities are necessarily true, since every reality must be consistent with itself," (p. 206)—does not apply to our conviction of necessity, since we may not be able to see what are the properties of real things; and therefore may have no conviction of their necessity. It may be a necessary property of salt to be soluble, but we see no such necessity; and therefore the assertion of such a property is not one of the necessary truths with which we are here concerned. But to turn back to the necessary or universal truths of geometry, and the ground of those attributes:—The main difference between the Author and the Reviewer is brought into view, when the Reviewer discusses the general argument which I had used, in order to show that truths which we see to be necessary and universal cannot be derived from experience. The argument is this,—

"Experience must always consist of a limited number of observations; and however numerous these may be, they can show nothing with regard to the infinite number of cases in which the experiment has not been made. . . . Truths can only be known to be general, not universal, if they depend upon experience alone. Experience cannot bestow that universality which she herself cannot have; nor that necessity of which she has no comprehension." (*Phil.* i. pp. 63, 64.)

Here is that which must be considered as the cardinal argument on this subject. It is therefore important to attend to the answer which the Reviewer makes to it. He says,—

"We conceive that a full answer to this argument is afforded by the nature of the inductive propensity,—by the irresistible impulse of the mind to generalize *ad infinitum*, when nothing in the nature of limitation or opposition offers itself to the imagi-

nation ; and by our involuntary application of the law of continuity to fill up, by the same ideal substance of truth, every interval which uncontradicted experience may have left blank in our inductive conclusion." (p. 207.)

Now here we have two rival explanations of the same thing,—the conviction of the universality of geometrical truths. The one explanation is, that this universality is imposed upon such truths by their involving a certain element, derived from the universal mode of activity of the mind when apprehending such truths, which element I have termed an Idea. The other explanation is, that this universality arises from the *inductive propensity*—from the *irresistible impulse to generalize ad infinitum*—from the *involuntary application of the law of continuity*—from the *filling up all intervals with the same ideal substance of truth*.

With regard to these two explanations, I may observe, that so far as they are thus stated they do not necessarily differ. They both agree in expressing this ; that the ground of the universality of geometrical truths is a certain law of the mind's activity, which determines its procedure when it is concerned in apprehending the external world. One explanation says, that we impress upon the external world the relations of our ideas, and thus believe more than we see,—the other says, that we have an irresistible impulse to introduce into our conviction a relation between what we do observe and what we do not, namely, to generalize *ad infinitum* from what we do see. One explanation says, that we perceive all external objects as included in absolute ideal space,—the other, that we fill up the intervals of the objects which we perceive with the same ideal substance of truth. Both sets of expressions may perhaps be admissible ; and if admitted, may be understood as expressing the same opinions, or opinions which have much in common. The Author's expressions have the advantage, which ought to belong to them, as the expressions employed in a systematic work, of being fixed expressions, technical phrases, intentionally selected, uniformly and steadily employed whenever the occasion recurs. The Reviewer's expressions are more lively and figurative, and such as well become an occasional composition ; but hardly such as could be systematically applied to the subject in a regular treatise. We could not, as a standard and technical phrase, talk of "filling up the intervals of observation with the same ideal substance of truth ;" and the "inevitable impulse to generalize" would hardly sufficiently express that we generalize according to

a certain idea, namely, the idea of space. Perhaps that which is suggested to us as the common import of the two sets of expressions may be conveyed by some other phrase, in a manner free from the objections which lie against both the Author's and the Critic's terms. Perhaps the mental Idea governing our experience, and the irresistible Impulse to generalize our observation, may both be superseded by our speaking of a Law of the mind's *Activity*, which is really implied in both. There operates, in observing the external world, a law of the mind's activity, by which it connects its observations; and this law of the mind's activity may be spoken of either as the Idea of space, or as the irresistible Impulse to generalize the relations of space which it observes. And this expression—the *laws of the mind's activity*—thus opposed to that merely *passive* function by which the mind receives the impressions of sense, may be applied to other ideas as well as to the idea of space, and to the impulse to generalize in other truths as well as those of geometry.

So far, it would seem, that the Author and the Critic may be brought into much nearer agreement than at first seemed likely, with regard to the grounds of the necessity and universality in our knowledge. But even if we adopt this conciliatory suggestion, and speak of the necessity and universality of certain truths as arising from the Laws of the mind's Activity, we cannot, without producing great confusion, allow ourselves to say, as the Critic says, that these truths are thus derived from *experience*, or from *observation*. It will, I say, be found fatal to all philosophical precision of thought and language, to say that the fundamental truths of geometry, the axioms, with the conviction of their necessary truth, are derived from experience. Let us take any axiomatic truth of geometry, and ask ourselves if this is not so.

It is, for example, an axiom in geometry that if a straight line cut one of two parallel straight lines, it must cut the other also. Is this truth derived or derivable from observation of actual parallel lines, and a line cutting them, exhibited to our senses? Let those who say that we do acquire this truth by observation, imagine to themselves the mode in which the observation must be made. We have before us two parallel straight lines, and we see that a straight line which cuts the one cuts the other also. We see this again in another case, it may be, the angles and the distances being different, and in a third, and in a fourth; and so on; and generalizing, we are

irresistibly led to believe the assertion to be universally true. But can any one really imagine this to be the mode in which we arrive at this truth? "We see," says this explanation, "two parallel straight lines, cut by a third." But how do we know that the observed lines *are* parallel? If we apply any test of parallelism, we must assume some property of parallels, and thus involve some axiom on the subject, which we have no more right to assume than the one now under consideration. We should thus destroy our explanation as an account of the mode of arriving at independent geometrical axioms. But probably those who would give such an explanation would not do this. They would not suppose that in observing this property of parallels we try by measurement whether the lines are parallel. They would say, I conceive, that we *suppose* lines to be parallel, and that then we *see* that the straight line which cuts the one must cut the other. That when we make this supposition, we are persuaded of the truth of the conclusion, is certain. But what I have to remark is, that this being so, the conclusion is the result, not of observation, but of the hypothesis. The geometrical truth here spoken of, after this admission, no longer flows from experience, but from supposition. It is not that we *ascertain* the lines to be parallel, and then *find* that they have this property: but we *suppose* the lines to be parallel, and *therefore* they have this property. This is not a truth of experience.

This, it may be said, is so evident that it cannot have been overlooked by a very acute reasoner, such as you describe your Critic to be. What, it may be asked, is the answer which he gives to so palpable an objection as this? How does he understand his assertion that we learn the truth of geometrical axioms from experience (p. 208), so as to make it tenable on his own principles? What account does he give of the origin of such axioms which makes them in any sense to be derived from experience?

In justice to the Reviewer's fairness (which is unimpeachable throughout his argumentation) it must be stated that he does give an account in which he professes to show how this is done. And the main step of his explanation consists in introducing the conception of *direction*, and *unity of direction*. He says (p. 208), "The *unity of direction*, or that we cannot march



from a given point by more than one path *direct to the same object*, is a matter of practical experience, long before it can by possibility become matter of abstract thought." We might ask here, as in the former case, how this can be a matter of experience, except we have some independent test of directness? and we might demand to know what this test is. Or do we not rather, here as in the other case, *suppose* the directness of the path; and is not the singleness of the direct path a consequence, not of its observed form, but of its hypothetical directness; and thus by no means a result of experience? But we may put our remark upon this deduction of the geometrical axiom in another form. We generalize, it is said, the observations which we have made ever since we were born. But this term "generalize" is far too vague to pass for an explanation, without being itself explained. We are impelled to believe that to be true in general which we see to be true in particular. But how do we see any truth? How do we pick out any proposition with respect to a diagram which we see before us? We see in particular, and state in general, some truth respecting straight lines or parallel lines, or concerning direction. But where do we find the conception of straightness, or parallelism, or direction? These conceptions are not upon the surface of things. The child does not, from his birth, see straightness and parallelism so as to know that he sees them. How then does his experience bear upon a proposition in which these conceptions are involved? It is said that it is a matter of experience long before it is a matter of abstract thought. But how can there be any experience by which we learn these properties of a straight line, till our thoughts are at least so abstract as to conceive what straightness is? If it be said that this conception grows with our experience, and is gradually unfolded with our unfolding materials of knowledge, so as to give import and significance to them: I need make no objection to such a statement, except this—that this power of unfolding out of the mind conceptions which give meaning to our experience, is something in addition to the mere employment of our senses upon the external world. It is what I have called the *ideal* part of our knowledge. It implies, not only an impulse to generalize from experience, but also an impulse to form conceptions by which generalization is possible. It requires, not only that nothing should oppose the

tendency, but that the direction in which the tendency is to operate should be determined by the laws of the mind's activity; by an internal, not by any external agency.

One main ground on which the Reviewer is disposed to quarrel with and reject several of the expressions used in the Philosophy;—such as that space is an Idea, a Form of our perception, and the like,—is this; that such expressions appear to deprive the external world of its reality; to make it, or at least most of its properties, a creation of the observing mind. He quotes the following argument which is urged in the "Philosophy," in order to prove that space is not a notion obtained from experience: "Experience gives us information concerning things without us, but our apprehending them *as* without us takes for granted their existence in space. Experience acquaints us with the form, position, magnitude, &c. of particular objects, but that they *have* form, position, magnitude, pre-supposes that they are in space." From this statement he altogether dissents. "No," says he, "the reason why we apprehend things *as* without us is that they *are* without us. We take for granted that they exist in space, because they *do* so exist, and because such their existence is a matter of direct perception, which can neither be explained in words nor contravened in imagination: because, in short, space is a *reality*, and not a mere matter of convention or imagination."

Now, if by calling space an *idea*, we suggest any doubt of its reality and of the reality of the external world, we certainly run the risk of misleading our readers; for the external world is real if anything be real: the bodies which exist in space are things, if things are anywhere to be found. That bodies *do* exist in space, and that *that* is the reason why we apprehend them as existing in space, I readily grant. But I conceive that the term Idea ought not to suggest any such doubt of the reality of the knowledge in which it is involved. Ideas are always, in our knowledge, conjoined with facts. Our *real knowledge* is *knowledge*, because it involves Ideas, *real*, because it involves Facts. We apprehend things as existing in space because they do so exist: and our idea of space enables us so to observe them, and so to conceive them.

But we want, further, a reason why, apprehending them as they are, we also apprehend, that in certain relations they could not be otherwise (that two straight linear objects could

not inclose a space, for instance). This circumstance is no way accounted for by saying that we apprehend them as they are; and is, I presume to say, inexplicable, except by supposing that it arises from some property of the observing mind:—an Idea, as I have termed it,—an irresistible Impulse to generalize, as the Reviewer expresses it. Or, as I have suggested, we may adopt a third phrase, a Law of the Mind's Activity: and in order that no question may remain, whether we ascribe reality to the objects and relations which we observe, we may describe it as “a Law of the mind's activity in apprehending what is.” And thus the real existence of the object, and the ideal element which our apprehension of it introduces, would both be clearly asserted.

I am ready to use expressions which recognize the reality of space and other external things more emphatically than those expressions which I have employed in the “Philosophy,” if expressions can be found which, while they do this, enable us to explain the possibility of knowledge, and to analyse the structure of truth. It is, indeed, extremely difficult to find, in speaking of this subject, expressions which are satisfactory. The reality of the objects which we perceive is a profound, apparently an insoluble problem\*. We cannot but suppose that existence is something different from our knowledge of existence:—that which exists, does not exist merely in our knowing that it does:—truth is truth whether we know it or not. Yet how can we conceive truth, otherwise than as something known? How can we conceive things as existing, without conceiving them as objects of perception? Ideas and Things are constantly opposed, yet necessarily co-existent. How they are thus opposite and yet identical, is the ultimate problem of all philosophy. The successive phases of philosophy have consisted in separating and again uniting these two opposite elements; in dwelling sometimes upon the one and sometimes upon the other, as the principal or original or only element; and then in discovering that such an account of the state of the case was insufficient. Knowledge requires Ideas. Reality requires Things. Ideas and Things co-exist. Truth is, and is known. But the complete explanation of these points appears to be beyond our reach. At least it is not necessary for the

\* These remarks were written in 1841. The preceding Essay contains a further discussion of this problem.

purposes of our philosophy. The *separation* of Ideas and Sensations in order to discover the conditions of Knowledge is our main task. How Ideas and Sensations are *united* so as to form Things, does not so immediately concern us.

I have stated that we may, without giving up any material portion of the Philosophy of Science to which I have been led, express the conclusions in other phraseology; and that instead of saying that all our knowledge involves certain Fundamental Ideas, the sources from which all universal truth is derived, we may say that there are certain Laws of Mental Activity according to which alone all the real relations of things are apprehended. If this alteration in the phraseology will make the doctrines more generally intelligible or acceptable, there is no reason why it should not be adopted. But I may remark, that a main purpose of the "Philosophy" was not merely to prove that there *are* such Fundamental Ideas or Laws of mental activity, but to *enumerate* those of them which are involved in the existing sciences; and to state the fundamental truths to which the fundamental ideas lead. This was the task which was attempted; and if this have been executed with any tolerable success, it may perhaps be received as a contribution to the philosophy of science, of which the value is not small, in whatever terms it be expressed. And this enumeration of fundamental ideas, and of truths derived from them, must have something to correspond to it, in any other mode of expressing that view of the nature of knowledge which we are led to adopt. If instead of *Fundamental Ideas*, we speak of Impulses of generalization, or of *Laws of mental activity*, we must still distinguish such Impulses, or such Laws, according to the distinctions of ideas to which the survey of science led us. We shall thus have a series of groups of Laws, or of classes of generalizing Impulses, corresponding to the series of Fundamental Ideas already given. If we employ the language of the Reviewer, we shall have one generalizing Impulse which suggests relations of Space; another which directs us to properties of Numbers; another which deals with Time; another with Cause: another which groups objects according to Likeness; another which suggests a Purpose as a necessary relation among them; to which may be added, even while we confine ourselves to the physical sciences, several others, as may be seen in the "Philosophy." Now when the fundamental conditions and elements

of truth are thus arranged into groups, it is not a matter of so much consequence to decide whether each group shall be said to be bound together by an Idea or by an Impulse of generalization; as it is to see that, if this happen in virtue of Ideas, here are so many distinct Ideas which enter into the structure of science, and give universality to its matter; and again, if this happen in virtue of an irresistible Impulse of generalization in each case, we have so many different kinds of Impulses of generalization. The main purpose in the "Philosophy" was to analyse scientific truth into its conditions and elements; and I did not content myself with saying that those elements are Sensations and Ideas; the Ideas being that element which makes universal knowledge conceivable and possible. I went further: I enumerated the Ideas which thus enter into science. I showed that in the sciences which I passed in review, the most acute and profound inquirers had taken for granted that certain truths in each science are of universal and necessary validity, and I endeavoured to select the idea in which this universality and necessity resided, and to separate it from all other ideas involved in other sciences. If therefore it be thought better to say that those Principles in each science upon which, as upon the axioms in geometry, the universality and necessity of scientific truth depends, are arrived at, not by Ideas, but by an irresistible Impulse of generalization, those who employ such phraseology, if they make a classification of such Impulses corresponding to my classification of Ideas, will still adopt the greater part of my philosophy, altering only the phraseology. Or if, as I suggested, instead of "Fundamental Ideas," we use the phrase "Laws of Mental Activity," then our primary intellectual Code—the Constitution of our minds, as it may be termed—will consist of a Body of Laws of which the Titles correspond with the Fundamental Ideas of the "Philosophy."

My object was, from the writings of the most sagacious and profound philosophers who have laboured on each science, to extract such a code, such a constitution. If I have in any degree succeeded in this, the result must have a reality and a value independently of all forms of expression. Still, I do not think that any language can ever serve for such legislation, in which the two elements of truth are not distinguished. Even if we adopt the phraseology which I have just employed, we shall have to recollect that Law and Fact must be kept dis-

tinct, and that the Constitution has its Principles as well as its History.

But I will not longer detain you by seeking other modes of expressing the Fundamental Antithesis to which the preceding Essay refers. The Remarks which I here send you were written three years ago, on the appearance of the Review which I have quoted. If I succeed in obtaining for them a few minutes' attention from you and a few other friends, I shall be glad that they have been preserved.

I am, my dear Herschel,  
always truly yours,

W. WHEWELL.

P.S. I have spared you a large portion of my Remarks as originally written. I had gone on to show that, in my "Philosophy," I had not only enumerated and analysed a great number of different Fundamental Ideas which belong to the different existing sciences, but that I had also shown in what manner these ideas enter into their respective sciences; namely, by the statement or use of Axioms, which involve the ideas, and which form the basis of each science when systematically exhibited. A number of these Axioms, belonging to most of the physical sciences, are stated in the "Philosophy." I might have added also, that I have attempted to classify the historical steps by which such Axioms are brought into view and applied. But it is not necessary to dwell upon these points, in order to illustrate the difference and the agreement between the Reviewer and me.

*Sir John F. W. Herschel, Bart. &c.*

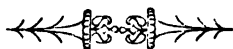
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